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I. A NEW PHASE OF PLANT-LIFE.*

It is interesting occasionally to look back to the time when Natural History, still in its unscientific stage, consisted of a catalogue of names, diversified, as regards zoology, with apocryphal anecdotes, stilted declamations on the nobility of the horse and the faithfulness of dogs, and with a *fœdissima proluvies* of twaddle about "instinct and reason." Botany was a still more meagre study, relieved merely with notes on the supposed healing virtues of the species described. In those good old days we believed in the existence of sharply-defined genera and orders, not to speak of species. We thought that all beings were duly labelled and pigeon-holed for our more convenient study. Even the time-honoured antithesis of "man and beast" seemed scarcely so glaring and so fundamental as the contrast between animals and vegetables. We could not, of course, refuse to regard the latter as living beings. We saw them developed from a germ, growing by the absorption and assimilation of foreign matter, secreting substances altogether different from the nourishment taken in, reproducing reds similar to that from which they sprung, and finally decaying and being resolved into unorganised compounds. But we regarded them, for all this, as entirely passive. They were held incapable of any other motion than that due to the passing wind. They were supposed to be nourished exclusively upon whatever matter—inorganic or merorganic—came casually in contact with their leaves or rootlets, having at most the somewhat negative power of a *veto*, by refusing to take in anything useless or injurious. Still more decidedly were they considered as insensient, devoid of all feeling,—as incapable of recognising and of reacting upon any contact or impulse from without as are the very stones by the wayside. True, the movements of

* Insectivorous Plants. By CHARLES DARWIN, F.R.S. London: John Murray.

the sensitive plant were known, and could not be explained away. But it is remarkable with what ease a fact can be shelved if unconnected, ill-understood, or not in harmony with received opinions. Had the public been informed half a century ago that plants exist decidedly carnivorous—capable of catching and killing insects, and of digesting their remains—scarcely any eminence on the part of the narrator would have saved his statement from being denounced as a fable. Yet in the work which lies before us there is the fullest proof that such is the case. Plants are found in many parts of the globe, our own country included, which depend for subsistence upon the insects which they catch. They are endowed with a sensibility probably more acute than that possessed by any part of the human body, and even discriminate between nutritive and innutritious substances.

It is very interesting that these revelations should be laid before the world by Mr. Darwin. Many authorities—from MM. Quatrefages, Milne-Edwards, and the late Agassiz, down to the Editor of the "Family Herald"—affect to treat our great naturalist as an amateur speculator who has amused himself by commenting upon the information collected by others. His work on "Insectivorous Plants" proclaims him—as has long been known to all candid and competent judges—a careful, patient observer and experimentalist, who, had his attention in early life been directed to physics or to chemistry, might have gathered in those fields of research laurels not less splendid than those he has won in the regions of organic science.

At the same time it must not be supposed that Mr. Darwin is the first and sole observer of every fact detailed in his work. That certain plants catch insects has been long known and commented upon ; but Darwin has observed and described, more fully and clearly than any previous author, the mechanism for capture, the circumstances under which it is brought into play, and, above all, has established the capital fact of the true digestion of the prey. Henceforth the existence and the attributes of carnivorous plants are points recognised in botanical science, and must be taken into account by all speculators on the life of plants and on their relations to animals. With characteristic modesty Mr. Darwin sums up his researches by saying—"We see how little has been made out in comparison with what remains unexplained and unknown."

The sun-dew (*Drosera rotundifolia*) is a plant not uncommon on heaths, and, though curious, is not remarkable either

for beauty or utility to man. Its leaves are covered with so-called tentacles,—hair-like filaments, generally standing upwards, and each carrying on its summit a gland which may be roughly compared to the head of a pin. These glands are "each surrounded by large drops of extremely viscid secretion, which, glittering in the sun, have given rise to the plant's poetical name of the sun-dew." This secretion, acting to some extent like bird-lime, is the primary agent in capturing insects. "When an insect alights on the central disc it is instantly entangled by the viscid secretion, and the surrounding tentacles after a time begin to bend, and ultimately clasp it on all sides. Insects are generally killed, according to Dr. Nitschke, in about a quarter of an hour, owing to their tracheæ being closed by the secretion. If an insect adheres to only a few of the glands of the exterior tentacles, these soon become inflected, and carry their prey to the tentacles next succeeding them inwards; these then bend inwards, and so onwards until the insect is ultimately carried, by a curious sort of rolling movement, to the centre of the leaf. Then, after an interval, the tentacles on all sides become inflected, and bathe their prey with their secretion in the same manner as if the insect had first alighted on the central disc. It is surprising how minute an insect suffices to cause this action: for instance, I have seen one of the smallest species of gnats (*Culex*) which had just settled with its excessively delicate feet on the glands of the outermost tentacles, and these were already beginning to curve inwards, although not a single gland had as yet touched the body of the insect."

To measure, as it were, the sensitiveness of the glands, and ascertain what were the smallest bodies capable of inducing motion in the tentacles, a number of delicate experiments were instituted. It was found that little bits of human hair, measuring only 8-1000ths of an inch in length and weighing only 1-78740th of a grain, caused the tentacles to curve inwards. The author remarks that a bit of hair 1-50th of an inch in length, and therefore much larger than those used in the above experiments, was not perceived when placed on his tongue. "It is extremely doubtful," he adds, "whether any nerve in the human body, even if in an inflamed condition, would be in any way affected by such a particle supported in a dense fluid, and slowly brought in contact with the nerve. Yet the cells of the glands of *Drosera* are thus excited to transmit a motor impulse to a distant point, inducing movement. It appears to me that hardly any more remarkable fact than this has been observed

in the vegetable kingdom." But this extreme sensibility is not promiscuous in its nature. It reacts to continuous pressure to the wonderful extent just recited, but a momentary touch, even several times repeated, produces no inflection. "On one occasion forty-five glands on eleven leaves were touched once, twice, or even thrice, with a needle or stiff bristle. This was done as quickly as possible, and with force sufficient to bend the tentacles; but only six of them became inflected—three plainly and three in a slight degree." Yet these tentacles, so indifferent to a passing touch, were quickly deflected if bits of meat were laid upon them, and were thus found to be in a normal, healthy condition. This comparative insensibility to touch, the author very justly argues, must be of advantage to the plant, since in windy weather its leaves must be occasionally brushed by the tall blades of grass growing near. "It would be a great evil if the tentacles were thus brought into action, for the act of re-expansion takes a considerable time, and until the tentacles are re-expanded they cannot catch prey." But prolonged pressure—even from utterly innutritious matter, such as glass, cinder, gold-leaf, cork, &c.—produces inflection. This is a serious consideration for teleologists, as furnishing proof that the plant is not perfectly adapted to the circumstances in which it is placed. "If a bit of dry moss, peat, or other rubbish, is blown on to the disk, as often happens, the tentacles clasp it in a useless manner. They soon, however, discover their mistake, and release such innutritious matter."

Drops of water falling upon the leaf—whether as natural rain or artificially sprinkled—produce no effect. This is the more remarkable as small rain-drops often adhere to the viscid secretion of the plant, and must occasion a pressure vastly greater than that of the bits of hair above mentioned. It is obvious that to be affected by rain-fall would be highly inconvenient to the plant.

The success of the sun-dew in capturing prey is great. "On one plant all six leaves had caught their prey, and on several plants very many leaves had each caught more than a single insect. On one large leaf I found the remains of thirteen distinct insects." Hence it would seem that the snares of the sun-dew are quite as efficient as a spider's web. As to the species captured it appears that flies (Diptera) are the commonest victims. "The largest kind which I have seen caught was a small butterfly (*Cœnonympha pamphilus*), but the Rev. H. M. Wilkinson informs me that he found a large living dragon-fly with its body firmly held

by two leaves." A kindred species, *Drosera filiformis*, very abundant in New Jersey, catches an extraordinary number of small and large insects—even great flies of the genus *Asilus*, moths, and butterflies.

The question now arises whether insects alight on the leaves by mere chance, as a casual resting-place, or whether there is some object of special attraction which serves as a bait for the trap. On this point Mr. Darwin suspends judgment. "I suspect," says he, "from the number of insects caught by the English species of *Drosera*, and from what I have observed with some exotic species kept in my greenhouse, that the odour is attractive." We should think that insects may be tempted to settle on the plant by the dew-like appearance of the secretion on the glands. Flies especially are well known to be "thirsty souls," and rarely lose an opportunity of drinking. Butterflies are also very fond of sipping moisture, as from the edges of shallow forest-pools.

But we must now enquire what becomes of the insects thus caught and killed? There are two possibilities: their bodies may pass into putrefaction, the plant being nourished by the liquid and gaseous products, or they may undergo a veritable process of digestion, as if they had been introduced into the stomach of an animal. Mr. Darwin has clearly shown that the secretions of the *Drosera* act on albumenoid compounds exactly as does the gastric juice of the Mammalia, the digested matter being afterwards absorbed. "This fact," he very justly remarks, "is a wonderful one in the physiology of plants. It is well known that the digestive process in animals requires the presence of an acid in company with the pepsin secreted by the stomach. If this acid is neutralised by an alkali, digestion is arrested, and goes on again if the alkali is again saturated by a suitable proportion of an acid. The very same results were obtained with the *Drosera*. Small cubes of albumen were given to the plant, and began to be dissolved in the ordinary manner. Small quantities of alkaline solutions were then added, so as to neutralise the acidity of the juices secreted by the plant. Immediately the action was brought to a stand-still, and the albumen was no longer attacked. If, now, traces of weak hydrochloric acid were added, so as to restore the secretion to its normal condition, the process of digestion was at once resumed. Within forty-eight hours from the time the acid was given the four cubes were not only completely dissolved, but much of the liquefied albumen was absorbed." Small portions of the ferment—removed

from the plant with due precaution and placed in a glass—were found able to dissolve fragments of coagulated albumen. In all the numerous experiments made on the digestion of cubes of albumen "the angles and edges were invariably first rounded off." This, according to Schiff (" Lecons Phys. de la Digestion," vol. ii., 1867, p. 149), is "a special characteristic of the digestion of albumen by the gastric juice of animals." When, on the contrary, such cubes are dissolved by chemical action, solution goes on over the whole surface in contact with the solvent.

Another very important point is that the secretion of the *Drosera*—like the gastric juice of the higher animals—possesses a decidedly antiseptic property, and, whilst effecting digestion, checks putrefaction. This interesting fact is proved by the following experiments :—" During very warm weather I placed close together two equal-sized bits of raw meat—one on a leaf of *Drosera*, and the other surrounded by wet moss. They were thus left for forty-eight hours, and then examined. The bit on the moss swarmed with Infusoria, and was so much decayed that the transverse striæ on the muscular fibres could no longer be clearly distinguished; whilst the bit on the leaf, which was bathed by the secretion, was free from Infusoria, and its striæ were perfectly distinct in the central and undissolved portion. In like manner small cubes of albumen and cheese, placed on wet moss, became threaded with filaments of mould, and had their surfaces slightly discoloured and disintegrated; whilst those on the leaves of the *Drosera* remained clean, the albumen being converted into a transparent fluid."

Among the substances thus dissolved and digested by the leaves of the *Drosera* may be enumerated roast meat, pure fibrin, areolar tissue, fragments of bone, and pure phosphate of lime. The latter, however, and also raw meat, are too powerful stimulants, and, except in very minute doses, injure or kill the leaves. Fat is not digested. Gelatine and isinglass have a comparatively feeble exciting action on the plant. "This," remarks Mr. Darwin, "is an interesting fact, as it is well known that gelatine by itself has little power of nourishing animals." The secretion of the *Drosera* quickly dissolves casein in the condition in which it is found naturally existing in milk. But the pure chemically-prepared casein is scarcely, if at all, attacked. Such casein we learn on high authority is almost entirely insoluble in the gastric juice of animals. This is another evidence in favour of the identity of the secretion of the *Drosera* with gastric juice,

since both act so differently on the fresh casein of milk and on the dry matter prepared by chemists.

Cheese was digested only to a slight extent, and proved injurious to the leaves. Legumen appears to be digested and absorbed. The pollen-granules of plants are penetrated by the secretion, and the contents partially digested. Gluten is partially acted on, but—like raw meat, phosphate of lime, or even albumen in large pieces—it is too powerful a stimulant. But gluten freed from starch, by steeping in very dilute hydrochloric acid, was readily digested, and was not found to injure the glands of the leaves in any marked degree.

Among the substances not acted upon by the secretion of *Drosera* are hair, bits of finger-nails, quills of feathers, mucin, urea, chitin (the chief constituent of the outward skeleton of insects), chlorophyll, cellulose, fats, oils, starch, sugar, gum, dilute alcohol, and vegetable extracts free from albumen. Here is further proof of the identity of the *Droseræ* ferment with gastric juice, since none of the substances just enumerated—as far as is known—are digested by the gastric juice of animals, though certain of them are acted upon by the other secretions of the intestinal canal.

The *Drosera* may be termed an omnivorous plant; for though its principal food, doubtless, consists of insects, it is also capable of deriving nourishment from the pollen of plants, which, doubtless is occasionally deposited by the wind upon its leaves. Even seeds, though not absolutely dissolved, are attacked by the secretion, and some principle is doubtless extracted out of them. It is at least certain that seeds placed upon the leaves of the *Drosera* produce inflection of the tentacles, and are subsequently found to have been injured. Of seven cabbage seeds thus treated three only were found capable of germinating, and of the three seedlings one speedily perished. Very similar results were obtained with the seeds of radish and cress. On the other hand, seeds of black mustard, celery, caraway, and wheat were found not to excite the plant more than inorganic objects. But the identity, or at least the close similarity, existing between the gastric juice of animals and the secretion of the *Drosera*,—two complex liquids subserving the same function, though elaborated by organisms so remote from each other,—though it may justly be styled "a new and wonderful fact in physiology," is not by any means the only marvel which the study of the *Drosera* reveals. We have already seen that a drop of water, falling upon the leaves of the plant, entirely fails in causing any

inflection of the tentacles; but the case is very different with fluids containing nitrogen, whether in the shape of organic compounds or of ammoniacal salts. Nine salts of ammonia which were applied to the plant, in the state of weak solution, all caused the inflection of the tentacles, and frequently also of the blade of the leaf. Of all the salts tried the phosphate is by far the most powerful stimulant. The 1-3840th part of a grain placed on the glands of the disc, so as to act indirectly upon the outer tentacles, produced this result. But this is far from being the limit. If the solution was applied for a few seconds directly to the gland of an outer tentacle, 1-153,600th of a grain was sufficient, whilst if the leaf was entirely immersed—allowing time for every gland to absorb all that it can—inflection was produced by the almost inconceivably small quantity of 1-19,760,000th of a grain. As, moreover, the phosphate of ammonia in question contains 35 per cent of crystalline water, the really efficient active matter in the solution would be, in round numbers, the 1-30,000,000th of a grain! This is a degree of sensitiveness far surpassing that of any method of analysis, with the exception of the spectroscope. Dissolve 1 grain of phosphate of ammonia in a 31-gallon cask full of water. Of this solution take ½ a drachm. The most sensitive reagents, in the hands of the most skilful chemist, will fail to show the presence of the salt. Yet the *Drosera*—a plant without any specialised nervous system, insensient in common belief as the clods of the valley—detects it at once. We see no reason here for suspecting any error. The salt, Mr. Darwin informs us, "was in some cases weighed for me by a chemist in an excellent balance." Now it is scarcely necessary to observe that balances of modern construction, and chemists of modern training, are required to weigh with accuracy much smaller amounts than 1 grain. The quantities of water in which this 1-grain dose was to be dissolved were many times measured with great care. The experiments were repeated during several years by Mr. Darwin himself and his two sons, who were at first as incredulous as himself; and simultaneous trials were made with other leaves, immersed in still weaker solutions and in pure water, by way of control. "I hope," adds the distinguished author, "that some one may hereafter be induced to repeat my experiments: in this case he should select young and vigorous leaves, with the glands surrounded by abundant secretion. The leaves should be carefully cut off and laid gently in watch-glasses, and a measured quantity of the solution and of water poured over

each. The water used must be as absolutely pure as it can be made. It is to be especially observed that the experiments with the weaker solutions ought to be tried after several days of very warm weather. Those with the weakest solutions should be made on plants which have been kept for a considerable time in a warm greenhouse or cool hothouse; but this is by no means necessary for trials with solutions of moderate strength."

It may, perhaps, allay the scepticism, especially of "anti-Darwinians," who may read this passage, if they reflect that this high sensitiveness of the *Drosera* is merely in harmony with other well-known facts. Professor Donders and Dr. de Ruyter, of Utrecht, find, from their experiments, that less than one-millionth of a grain of sulphate of atropia, in an extremely diluted state, if applied directly to the iris of a dog, paralyses the muscles of this organ. The odorous particles which, floating in the air, are detected by the olfactory nerves, must be vastly smaller than the minutest dose of phosphate of ammonia recognised by the *Drosera*.

With a view to ascertain the seat and the nature of the sensitiveness observed in this remarkable plant, a series of careful experiments have been instituted with a variety of agents. Some of them produced a poisonous influence, but others which have a powerful action upon the nervous system of animals, produce here no effect. Hence it is concluded that—"the extreme sensibility of the glands, and their power of transmitting an influence to other parts of the leaf, causing movement, a modified secretion or aggregation does not depend on the presence of a diffused element allied to nerve tissue." This was shown by a variety of interesting experiments. The leaves of the *Drosera* were brought in contact with a variety of narcotics. Several of these, "which act powerfully upon the nervous system of animals, produce no effect upon *Drosera*." We may in particular mention the poison of the cobra, so well-known for its rapid and deadly action upon the nerve-centres of animals. But prolonged immersion in this poison, far from checking, appears rather to stimulate "the spontaneous movements of the protoplasm in the cells of the tentacles." Hence, then, we have, in this plant, sensation and the transmission of impulse not merely without demonstrable nerves, but apparently without nerve-tissue, and in like manner we have movement without muscular fibre! Concerning the mechanism of these movements, and the nature of the impulse, our knowledge is still

very rudimentary. Yet the mere fact that such movements and such impulses exist, is surely most significant.

But the various species of *Drosera* are far from being the sole insect-catching and flesh-digesting vegetables. In North Carolina grows the *Dionæa muscicapa*, "Venus's fly-trap," a carnivorous plant still more highly specialised. This plant does not entangle its prey by the aid of an ever-ready glutinous secretion, but closes upon it like a spring trap. The leaf has two lobes standing at the inclination of rather less than a right angle to each other. "Three minute pointed processes or filaments, placed triangularly, project from the upper surfaces of both, and are remarkable from their extreme sensitiveness to a touch. The margins of the leaf are prolonged into sharp, rigid projections, which I will call spikes, into each of which a bundle of spiral vessels enters. The spikes stand in such a position that when the lobes close they interlock like the teeth of a rat-trap. The midrib of the leaf, on the lower side, is strongly developed and prominent." The leaf, unlike that of *Drosera*, is sensitive to the momentary touch of a solid body, but is also unaffected by drops of water or currents of air. As soon as one of the filaments is touched, the lobes close with remarkable quickness, and the marginal spikes interlace. So firmly are the lobes thus pressed together, that if any large insect has been caught a corresponding projection on the outside of the leaf is distinctly visible. When closed, they resist reopening with an astonishing force, and are generally ruptured before yielding. But if pulled asunder without being torn, they close again, according to the statement of Dr. Canby, "with quite a loud flap." Though this plant makes no use of any viscid secretion to capture its prey, yet when an insect is once enclosed a fluid is poured out analogous to that secreted by the *Drosera*, but more decidedly acid, and a process of digestion and absorption sets in. If, however, a leaf has closed over any non-nitrogenous, and consequently innutritious, matter, such as wood, cork, moss, or paper, the leaf remains quite dry, and soon re-expands. Over a nitrogenous body, on the contrary, the leaf remains closed for many days, and when it re-opens it is still torpid, and never acts again, or at most only after a considerable lapse of time. The insects caught differ from those generally captured by the *Drosera*. This latter plant, aided by its bird-lime, is able to secure the tiniest and most rapidly-flying insects. Out of fourteen leaves of the *Dionæa* containing captured insects, three had caught ants, five

Elaters, two *Chrysomelas*, one a *Curculio*, one a thick and broad spider, one a *Scolopendra*, and one a fly. Thus the plant may be said to prey upon apterous species, or upon such as, though winged, cannot instantly take flight when the lobes of the leaf close. Very small insects may escape between the spikes, and some very strong species may succeed in forcing their way out. This was once observed by Mrs. Treat in the case of a species of rosechafer (*Macrodactylus subspinosus*). As regards the range of its digestive powers, *Dionæa* corresponds very closely with *Drosera*. As regards the movement of the leaves, we cannot omit to mention Dr. Burdon Sanderson's beautiful discovery that there is a normal electric current in the blade of the leaf and in its foot-stalk; when the leaf is irritated, the current is disturbed in the same manner as takes place during the contraction of the muscles of an animal. It is noteworthy that *Dionæa* is a less prosperous member of the plant world than is *Drosera*. Whilst the latter has been developed into about 100 species ranging in the Eastern Continent from the Arctic regions to South Africa, India, and Australia, and in the Western from Canada to Tierra del Fuego, *Dionæa* forms only a single species, limited to one district in Carolina.

Our attention is next drawn to the *Aldrovandas*, small aquatic plants allied to the Droseraceæ, and capable of securing and preying upon living creatures. Here also we find the secretion of a true digestive fluid and subsequent absorption of the matter thus digested. But in these species we find processes which appear also to absorb excrementitious and putrescent animal matter. "If this view is correct," says Mr. Darwin, "we have the remarkable case of different parts of the same leaf serving for very different purposes, one part for true digestion, and another for the absorption of decayed animal matter. We can thus also understand how, by the gradual loss of either power, a plant might be gradually adapted for the one function to the exclusion of the other." In the genera *Utricularia*, *Genlisea*, and their allies, we find, accordingly, that though animals are entrapped, they are not digested, but allowed to pass into putrefaction, upon the products of which the plant is nourished. Mr. Darwin's final summary may be usefully quoted in full:—

"Ordinary plants of the higher classes procure the requisite inorganic elements from the soil by means of their roots, and absorb carbonic acid from the atmosphere by means of their leaves and stems. But we have seen that

there is a class of plants which digest and afterwards absorb animal matter, namely all the Droseraceæ, Pinguicula, and, as discovered by Dr. Hooker, Nepenthes, and to this class other species will almost certainly soon be added. There is a second class of plants which, as we have just seen, cannot digest but absorb the products of the decay of the animals which they capture, namely, *Utricularia* and its close allies; and from the excellent observations of Dr. Mellichamp and Dr. Canby there can scarcely be a doubt that *Sarracenia* and *Darlingtonia* may be added to this class, though the fact can hardly be considered as fully proved. There is a third class of plants which feed, as is now generally admitted, upon the products of the decay of vegetable matter, such as the bird's-nest orchis (*Neottia*). Lastly there is the well-known fourth class of parasites (such as the mistletoe), which are nourished by the juices of living plants. Most, however, of the plants belonging to these four classes obtain part of their carbon, like ordinary species, from the atmosphere."

A writer unknown, criticising Mr. Darwin's work in a daily paper, winds up an otherwise not unfair notice with the following strange comments:—

" Mr. Darwin forbears to connect his discoveries in this direction with his general theory, and we have no wish to challenge his adherents on the subject. But the existence of these insect-eating plants does seem to us to militate against the theory of infinitesimal changes, each perpetuated by its beneficial action on the life of the species. For it is only when the tentacles and filaments and valves which render seizure possible have become so far complete as to capture at least a few insects, and when the powers of digestion have been acquired, that the plant can benefit by these exceptional developments. And by what influences were the successive changes in this direction sustained and increased, while of no use, till they reached the point of high elaboration at which they become useful, except by that very intellectual providence and controlling purpose which it is the paramount object of Darwinism to exclude ? "

Now by what exact stages the *Dionæa* and the *Drosera* have reached their present stage of development it would be utterly premature to pronounce. But we see, in the case of the *Drosera*, that the adaptation is imperfect—a feature intelligible if it has been attained by a process of evolution, but scarcely conceivable on the hypothesis of special creation. Whoever reads Mr. Darwin's work will

find that many plants are able to destroy insects without the somewhat elaborate arrangements which we recognise in the *Drosera* and *Dionæa*—that some plants derive no benefit at all from the victims, and that others, advancing a step higher, utilise the products of their decaying remains as manure. So that plants can benefit by these "exceptional developments" *before* "powers of digestion have been acquired." Nor, therefore, have any mysterious "influences" been required to sustain and increase the successive changes in this direction, since we see that they may have been of use long before their present point of high elaboration was reached. The concluding sentence is worthy of a Caccini. The "paramount object of Darwinism"—or rather we might say the sole object—is to elucidate the origin and existence of species. The uses or abuses to which it may be put—whether by theologians or anti-theologians—are independent issues with which the naturalist, as such, is not concerned.

What reply can the advocates of Specialism furnish if challenged to point out some good reason why the *Drosera* and the *Dionæa* should have been chosen for endowment with the powers of catching and digesting animal prey? If the object were to thin a redundant insect population, surely some more common plant would have better answered the purpose than one which, like the *Dionæa*, appears to be dying out. On this subject the reviewer quoted remarks:— "Suggesting to us rather their character as monstrosities, exceptions to the general tendency of nature, which must naturally be rare." This notion is on a level with the theory that fossil remains were *lusus naturæ*. "Monstrosities,"—as applied to an entire species,—"general tendency of Nature," are phrases which betray an utterly unscientific, or rather anti-scientific, vein of thought.

II. VEGETARIANISM: THE GREAT DIETETIC REFORM.

A GARRULOUS old man, who long ago came under our observation, was one day relating to a group of haymakers his own early adventures. One of the audience quietly chalked down on a tree-trunk the number of years which this rural Münchausen—according to his own version—had passed in different parts of the world, and at last greatly amused the audience and confounded the speaker by exclaiming that the total reached the very patriarchal figure of three hundred and eighty-four years. In like manner it should be a consolation for us to know that, plentiful as evil is in the world, it can be accounted for many times over. Different bodies of world-betterers have traced it all to its sources, and can point out the way to its abolition, if mankind would only follow. The teetotaller assures us that if we will but consent to abstain altogether from alcoholic beverages, at least 60 per cent of the existing poverty, vice, crime, disease, and so forth, will forthwith cease to exist. The crusaders against the use of tobacco trace a more modest portion, say 10 per cent, to the use of the American weed. Enthusiastic educationists contend that full one-half of the above-mentioned evils spring either from ignorance or from knowledge not instilled in the authorised place or by the authorised persons. A certain body of sanitary reformers contend that if we would only convert half England into an irrigation-farm a long step towards the golden age would be taken, and another large percentage of poverty, disease, and death would disappear. In Dr. Richardson's model city, Hygeia, the death-rate is to fall to five per thousand, and we presume a corresponding decrease of sickness and debility is to be insured. Political economists are disposed to attribute a very large portion of existing evils to the neglect of saving, to "unorganised" charity, and to the giving of out-door relief. The Malthusians ascribe a preponderating amount of whatever is undesirable to the too rapid increase of population. The "Shrieking Sisterhood" maintain, *con strepitu*, that the decay of empires, the want of public and private morality, and the low standard of intelligence are due to the exclusion of women from the franchise, from Parliament, and from the learned professions. Whilst we listen in amazement to these conflicting oracles, who jointly, if we may believe

their statements, account for much more than 100 per cent of existing evils, we are saluted by a fresh voice. A band of philanthropists boldly tell us that the world is to be regenerated by a change of diet; that our woes—moral, physical, and economical—spring from the consumption of animal food; the very taste for alcoholic drinks, they declare, would leave us if we would but abjure beef and mutton and live upon the vegetable productions of the earth. It is difficult to calculate the extent of the changes which this reform would involve. We may form some idea of a country under the operation of the Maine Law or the Permissive Bill; but it is scarcely possible to imagine the trades which would be affected, and the interests which would suffer from a general—voluntary or compulsory—abjuration of animal food.

It may not be uninteresting to examine the various hopes of improvement and benefit held out by vegetarian advocates, and consider in how far they are likely to be realised, and also to consider some of the arguments advanced to prove that man, in making use of animal food, violates the "laws" of his nature.

Let us first turn to the economical phase of the question. Vegetarians contend that on their system an individual or a family may be supported more cheaply than on a mixed diet, and that their "reform" would enable a given country to maintain in comfort a larger population than is now practicable. The latter and more important of these propositions we will now examine.

It may be granted that 100 acres of fair lowland soil will support a greater amount of human life if planted with wheat, potatoes, and other crops directly consumed by man, than if it were laid out in permanent pasture or set with Italian rye-grass, with mangolds, Swedes, and vegetables intended for the food of cattle. It is plain that a certain amount of waste must take place, and that all the nutritious matter taken up by sheep and oxen from their pasturage does not re-appear, weight for weight, in the butcher's shop, in the form of mutton and beef. But unfortunately there is in England—and more or less in all countries—abundance of land which, from the shallowness and nature of the soil and the great elevation above the sea-level, is unfit for tillage. A great part of this land, however, yields excellent pasturage, and is at present utilised for grazing. Under a general and consistent vegetarian *regime* these lands would be thrown entirely out of use, and must cease to yield their quota to the food-market.

The woods and the moorlands produce, on our present system, a certain amount of human food in the shape of game. This amount may not be very great,* but it is still an item in the list of production, and if animal diet be proscribed it must entirely disappear.

A far more serious consideration is the vast stock of nourishment for which we are now indebted to the world of waters, and which we must renounce if vegetarianism is to prevail. Fishes, crustaceans, mollusca, certain amphibians, sea-birds, and even aquatic mammalia, enter more or less abundantly into the diet of every maritime population. Of the total amount of food thus obtained it is difficult to form an adequate conception. According to Simmonds† the total yearly produce of the cod-fisheries of the North-American coast alone amounts to 1,500,000 tons of fresh fish. Let us suppose that only one-half of this is fit to be eaten, and we have 750,000 tons of food which vegetarians ask us to reject. Unless they can show us how to make the ocean yield us an equivalent amount of vegetable matter, we can only reply "*Non possumus.*"

Hence, then, we fear that the economical advantages which vegetarianism seems to offer, if we look merely to rich arable soils, will fade away if we take the whole of the globe—both land and water—into our consideration.

Very probably vegetarian advocates will take exception to that portion of our argument which refers to the disuse of mountain grazing-ground. They will urge that, though they reject flesh, they admit milk, butter, and cheese as a part of their diet, and that these pasture-lands will therefore be made available as heretofore. We will not here, in reply, stay to discuss their consistency in thus sanctioning the use of matters purely animal—a concession, by the way, which improves their cookery much more than their logic. The farmer, at present, finds it profitable to keep cattle, and to dispose of the milk, cheese, and butter at certain prices; but in doing all this he works under conditions which in a vegetarian country would not exist. When a cow ceases to give milk in remunerative quantity he fattens her for the market. The surplus bulls he can at present utilise as veal in their early days, or as oxen when fully grown; but under the vegetarian system he must either let the aged cows and the surplus bulls live as pensioners for the rest of their lives, or he must—if permitted—destroy them without finding a market for their flesh. In either case he will

* About 70,000,000 rabbits are consumed annually in France.

† Waste Products and Undeveloped Substances, p. 156.

sustain a considerable loss. The consequence will be that he must ask for milk, butter, and cheese prices practically prohibitive, and if he cannot obtain these he must abandon cattle-keeping, and allow the grazing-lands to go out of use.

But the derangement which would flow from the general adoption of vegetarianism would not be restricted to articles of diet. There are many well-known substances of great economical and industrial importance which we obtain either directly, by the slaughter of animals, or under circumstances which would be greatly modified if such slaughter were prohibited. Thus train and cod-liver oils, tallow, hides, and hair could scarcely be procured in a consistenily vegetarian country. Even wool would undoubtedly become scarce and dear if the market for mutton were closed.

The refuse of the fisheries is rising into importance as a manure fully equal to Peruvian guano. But if fish might no longer be captured the supply of this fertiliser would be cut off, unless, indeed, the destruction of animal life for purposes other than food received an exceptional sanction. Even then the cost of the raw material would be greatly enhanced. There is, finally, another light, in which the abandonment of the fisheries would have to be regarded. In the past and the present they have afforded maintenance to a hardy maritime population, and have been justly considered a most valuable school for seamen. Surely it would be a mistake to close this school, and to put an end to the trade on which that population depends.

Finding thus little prospect of economical advantage from the general adoption of the vegetarian system, let us turn to a yet more important sphere, and ask—Would the rejection of animal diet render man morally better? We have here the opportunity to apply the "method of variations." If flesh-eating is the main cause—or even one of the main causes—of crime and vice, we shall assuredly find the criminality of a nation increase in the same proportion as the amount of animal food it consumes, other things being equal. Now we are bound to admit that other things in this case are far from equal. The question is complicated by differences of race, religion, and government, to say nothing of minor causes. Still a careful examination can scarcely fail to disclose the moral influences of diet, if such exist. Turning to the extreme north we find the Greenlanders and Esquimaux, a race who, from the necessities of their position, are purely carnivorous. In fact—with the exception of a little scurvy grass, used more as medicine

than as diet—they can scarcely be said to partake of any vegetable matter at all. Now if an animal diet produces the effects alleged by vegetarian advocates, we ought to find these people the most vicious—and especially the most bloodthirsty—human beings on the earth. We must further remember that, except in the few places where they have come in contact with European missionaries, no influences have been at work which might correct the supposed evil tendencies of an exclusively animal diet. Yet the facts are quite otherwise. The "Skraeling" is doubtless a low, dirty savage, but he is by no means remarkably ferocious or bloodthirsty, nor even very pugnacious. Europeans wrecked on the inhospitable shores inhabited by these tribes have very rarely met with ill-treatment. Where the Esquimaux come in collision with the Red-skins* of North America they rarely attempt to defend themselves against the aggressions and outrages of the latter, but quietly withdraw farther and farther to the north.

Let us next look at the Red-skins themselves. They are by no means a mild race, and may even deserve to be called ferocious and sanguinary. That they are carnivorous we of course admit; but they are far less exclusively carnivorous than the Esquimaux, since a part at least of their diet consists of the wild fruits of the country and of maize. Here, then, we see among two savage races the one which is purely flesh-eating far less pugnacious and bloodthirsty than the one which enjoys a mixed diet.

Going further southwards we find the ancient Mexican empire infamous for its human sacrifices and other cruelties exercised under the name of religion. Now the Mexicans of old, if not vegetarians, were far from being exclusively flesh-eaters. Agriculture and horticulture flourished among them; and we must remember that a settled people who have no domestic animals such as the ox, sheep, or pig, will always be compelled to subsist mainly upon vegetable produce. Here, then, we have another people less carnivorous than the native tribes on their northern frontier, but certainly not more gentle and merciful. In South America we have a very instructive instance. In the vast plains of La Plata we find the Guachos, one of the most purely carnivorous peoples on the globe, and certainly reckless of human life and ready with the dagger; but across the Andes

* This term is inelegant, but we use it under compulsion. To call the aborigines of America "Indians" is a blunder which is confusing, and for which there is no excuse.

we find, in Chili, men of the same race, and placed under very similar institutions, but using an abundance of vegetable food. Now the Chilian peasant is every whit as vindictive and as murderous as his neighbours east of the Andes. Crossing the Atlantic we glance at Dahomey and Ashanti, countries literally reeking with wanton bloodshed; yet animal matter does not form the whole, or even the preponderating part of their diet. The Malay is well known for his vindictive disposition and his proneness to assassination; but he comes in the selection of his food far nearer the vegetarian standard than many milder races. It is a significant fact, to which we may have to revert, that among savages cannibalism is most prevalent where animal food is sparingly accessible. In Europe we find the proportion of flesh consumed varies greatly. England, Holland, France, Germany, and Scandinavia are more carnivorous than Spain or Italy. But in England, with all its faults, respect for human life is far greater and assassination far less frequent than in Spain.

Hence we must conclude that a mainly or partly animal diet does not in man necessarily produce ferocity and bloodthirstiness any more than a vegetable regimen involves gentleness and humanity. It is not even proved that the butcher, the poulterer, the fisherman, the sportsman, and the angler are at all more cruel and vindictive in their dealings with their fellow-men than are persons who have never taken animal life. We are indeed, on the opposite side, reminded of some vegetarians who were noted for their extreme benevolence. Of this class we may take Shelley as the type; but the question arises—Was Shelley humane and benevolent from his vegetarian habits, or was he not rather a vegetarian from his excessive benevolence? We hold the latter view.

Vegetarians, however, try to make out their case by referring to the lower animals, and enlarging on the ferocity of the Carnivora and the mildness of the Herbivora. Unfortunately this alleged mildness is a myth. The elephant is a vegetarian "pure and simple;" yet a "rogue" elephant will, without the smallest provocation or necessity, attack and kill men, horses, or other animals whom he meets. One at least of the African species of rhinoceros is given to wanton aggression, and is more dreaded by travellers than the lion. Among the ruminants it is hard to name a species which, if gifted with the needful strength, will not attack man. The malevolence of the common bull is well known. The common buffalo of Southern Europe and Asia

is never to be trusted. The wild cattle of Lyme and Chillingham, and those of Lithuania, take the offensive whenever the opportunity occurs. As for the Cape buffalo few beasts of prey can surpass him in ferocity. He-goats and rams not unfrequently attack passers-by. The red deer, at certain seasons of the year, cannot be approached with safety. Several of the larger kinds of antelopes are of an aggressive disposition. The larger baboons, though feeding upon fruits, nuts, and vegetable matter, generally are decidedly ferocious. Very similar is the case with vegetarian birds. An old male ostrich is decidedly to be shunned. The common cock, the turkey cock, and the gander are quarrelsome and aggressive creatures, and will occasionally indulge in wanton attacks. An "auerhahn" (*Tetrao urogallus*) once flew at us with such pertinacity that we were able to take him prisoner. From these instances—which, if needful, might be greatly multiplied—we conclude that diet has very little to do in determining the ferocity or the mildness of an animal. Both herbivorous and carnivorous beasts, if attacked or annoyed, will resist with equal determination. If anyone doubts this let him offend an elephant, and "make a note" of the result.

Vegetarians advance yet one more argument. They tell us that a young tiger brought up in captivity, and fed on milk, bread, and cooked meat, will be "mild and gentle," but that if by accident it takes blood or raw flesh its "ferocity" is immediately developed. We admit the facts, but we demur to the interpretation. So long as the tiger has never tasted blood it remains in ignorance that human beings are articles adapted to its taste. As soon as it has had such a meal its sense of smell informs it that men consist of matter similar to what it has already eaten with relish. But where is the "ferocity" in this? Is there really more ferocity in eating animals than plants? Is the *Dionæa muscicapa* to be regarded as malignant, in comparison with ordinary plants?

We therefore feel warranted in inferring that there is no reason to expect any diminution of bloodthirstiness and ferocity from man's adoption of a purely vegetable diet: that he would be morally improved by such a change in any other respect there is not the slightest evidence. Further, even supposing that the rejection of animal food would decrease man's "combativeness," as the phrenologists call it, we are by no means sure that this would be an unmixed benefit.

Here is the place to examine the Vegetarian assertion

that drunkenness—or rather the appetite for alcoholic drinks—is merely a consequence of the use of animal food. Now, that a man who is confined to a dry animal diet may feel more thirst than one who feeds mainly upon juicy fruits will not be disputed: but the craving for alcohol is not essentially connected with thirst; it is merely one manifestation of a tendency felt more or less among all people quite irrespective of diet—the love for narcotics. Whether this desire is to be gratified with alcohol, opium, *Cannabis indicus*, betel, or coca, is a mere accident of locality. Now it certainly cannot be contended, with any respect for facts, that the nations which make the nearest approach to vegetarianism are any the less inclined to lap themselves in a temporary elysium, by the use of some drug of this class, than the most carnivorous tribes. Nor is alcohol itself scorned by people whose food consists chiefly of vegetables. The African negro—fed mainly on bananas and maize—quaffs rum as eagerly as the carnivorous red man of North America.

We must now ask what, if any, are the physical benefits to be derived from a purely vegetable diet? Vegetarians contend that by rejecting animal food we should prolong our lives, and increase in strength, health, and capacity for work, whether intellectual or muscular. Such assertions are equally difficult to prove and to refute. If we could divide the people—say of England, or of France, or of Germany—into two sections, each comprising one-half of every class, rank, and occupation, and could feed the one for some few generations on a strictly vegetable regimen, whilst the other were permitted to adhere to its present diet, we might then obtain results capable of deciding the question; but the facts and arguments now brought forward are lamentably defective.

Some few individuals up and down England may have found or fancied themselves the better in health after bidding a last farewell to beef or mutton. We do not dispute the possibility of such cases; but we have not the slightest doubt that quite as many persons could be found who would be benefitted by a more liberal supply of sound animal food. We have known persons who—in the days of '48, when the millennium was to have been suddenly brought in by revolutions and "movements," and articles in the *People's Journal*—gave the vegetarian system a trial. Some of these experimentalists held out for four or five months, others for as many years; but none of them could frankly declare that they experienced any tangible improvement in health,

strength, or endurance, and all have since returned to the ordinary diet of their country.

Vegetarians sometimes tell us of particular classes of men in foreign countries who take very little animal food, or none at all, and yet show a wonderful strength and endurance. Such are the porters of Constantinople and the men who—in the mines of Chili—carry up the ore "to grass" upon their backs. That these men are strong and vigorous we do not dispute, but that their strength and vigour are due to their abstinence from flesh, and would be decreased by the adoption of a mixed diet, is nowise proved. These occupations are generally in the hands of something very like a hereditary caste, and we know to how great an extent strength is hereditary. None but hale and hearty men could, at the beginning, enter upon such kinds of work. Those who were at all deficient in strength would either be killed off or would abandon the task, and thus, by a process of "natural selection," a body of men would be formed of remarkable strength, with little reference to their peculiar fare. All that these examples prove, at the utmost, is that a vegetable diet is not under certain circumstances incompatible with health and strength. But what would be the result of taking an invalid, or even an ordinary person in average health, and feeding him—as the Chilian miners are said to be fed—exclusively upon beans and water? We should like to see a few such experiments made, for public instruction, *in corpore vile*—*i.e.*, upon the wife-beaters, garotters, corner-men, and other the like beings who, worse than useless at present, might thus be made of some service to mankind. But we fear the investigation will never be undertaken in face of the rampant "humanitarians" of modern England.

To return from this digression, we have known vegetarians actually argue in this wise :—The camel is more enduring than the lion or the tiger. Therefore if man would, like the camel, eschew flesh he would become much more enduring than he is at present! There can surely be no need to point out the fallacy of such reasoning. But a counter argument, of equal validity, might be easily framed. Thus we might say that the grizzly bear has greater strength than the bison. Hence if man would adopt a purely animal diet he would greatly improve in strength. The fact is that strength and endurance, in very varying degrees, can be found both among Carnivora and Herbivora. Those creatures which excel most in a short display of strength or speed—such as the lion, tiger, horse, python, &c.—are generally least fitted for protracted exertion.

Let us now proceed to the considerations which indicate that a mixed diet is more beneficial to man in a physical point of view than an exclusively vegetable regimen. No vegetarian nation has ever made any considerable figure in the world. Those, on the contrary, which have been foremost in civilisation, and exercised the greatest influence, have been semi-carnivorous.* Now if animal food were unfavourable to health, vigour, longevity, and rapid increase of population, the very contrary results might be expected. We should see the flesh-eating nations decay spontaneously, or at all events be easily overthrown, whenever they came in collision with vegetarian races. But of such events history gives us no instance. Further, the littoral populations of Western and Northern Europe have always been, to a great extent, fish-eaters; yet they have been eminently hardy and vigorous, and remarkable for longevity. The women of the fishing-villages are often possessed of masculine strength and endurance. Can these facts be easily reconciled with the notion that animal food is injurious or debilitating? In modern England the upper classes—and especially the territorial aristocracy—appear more long-lived than the lower orders; yet they are assuredly not vegetarian in their diet. Again, the trainers of athletes—men certainly not given to forming theories, but guided simply by the results of experience—always insist upon the use of a mainly animal regimen as a preparation for any display of unusual strength or endurance. They have found that fruits, roots, and the like, do not give the support needed by the pedestrian, the race-runner, the oarsman, swimmer, or wrestler. In like manner our medical authorities, when strength is declining, find that it is best kept up by animal matters, such as fresh broiled meat, *extractum carnis*, cod-liver oil, &c.

Taking our stand on such facts we may challenge the vegetarian advocate to point out in what manner, or from what reason, animal food should be less salutary than a vegetable diet. What hurtful matter does it contain? It seems, indeed, to be pre-eminently adapted for the food of the highest-known form of animal life. We see the vegetable world collecting from inorganic matter, and from the decomposing *débris* of former life, certain principles, and elaborating them to a certain extent. The plant then becomes the nourishment of animals, and in their system the

* It is remarkable that our "six-footed rivals," the ants,—the only creatures who at all approach us in civilisation,—are, like ourselves, omnivorous. The animal restricted to one class of food must find itself placed at a disadvantage in the struggle for existence.

processes of selection, concentration, and elaboration are carried still further. The animal then serves for the food of men. We know that not merely the ultimate, but even the proximate, constituents of plants and of animals are very similar. There are vegetable albumenoids closely analogous to those of the animal system. There are vegetable and animal fats scarcely to be distinguished from each other. It is even true that vegetable substances—such as peas, beans, and lentils—may contain a larger percentage of combined nitrogen than the flesh of animals. But as a rule animal matters can be digested and assimilated more easily, with a less expenditure of the resources of the system, than their vegetable analogues. All this points in a direction opposite to the vegetarian theory. But these dietetic reformers cannot openly commit themselves to the view that animal matter is innutritious, indigestible, or injurious, because they tolerate, after all, the use of milk, butter, cheese, and eggs! To declare milk unnatural and injurious as food for a mammalian animal has hitherto seemed to them a somewhat daring statement. But cheese is certainly a less digestible food than beef or mutton, and has even been pronounced one of our national dietetic sins. Yet once admitting these favoured animal substances, the difficulty is to draw a logical boundary. If the egg, why not the chicken? The two are one and the same being, in two different stages of development. The only tangible distinction—viz., that the egg can be eaten without inflicting pain—is, if true, scarcely any ground why its action on the system of the eater should be modified. We are told that milk is a secretion expressly intended for food. Be it so; but to assert that the flesh of the ox is not equally adapted to be the food of man is simply to beg the question at issue. It is even possible to go further. The milk of the cow is undoubtedly the natural food of the calf, but why should it, any more than beef, be pronounced the natural food of man? To rob the calf of its mother's milk is, from a sentimental point of view, perhaps as great an outrage—as decided an infringement of the "normal arrangements of Nature"—as to rob it of its life. To compel a female animal, by artificial treatment, to give milk when she is not suckling her young is likewise "unnatural." It is quite possible that the secretion so obtained may assume a morbid character, and may convey the seeds of consumption, or at least of an impaired vitality, to the human beings by whom it is consumed. Pus cells have frequently been detected in milk. Thus one of the few kinds of animal matter which

the vegetarian tolerates is quite as likely to be injurious as those which he denounces and avoids.

Flesh, we are told, is not the "natural food" of man. In reply, we beg for an explanation of the apparently simple phrase "natural food." If we consider every animal species as a something unalterably fixed, destined to inhabit some particular region and to feed upon some especial article, for obtaining and digesting which it has been pre-adapted, the words have a very definite meaning; but if we regard the species as something mutable alike in its organisation, its habitat, and its diet, the natural food of any animal signifies merely the class of substances which we find it for the time being in the habit of consuming. Such substances need not be the best possible, but may often be merely the best procurable. An analogous feature in vegetable life has been pointed out by Dean Herbert. We must not suppose, *e.g.*, that if we find a plant growing on moorlands, that moorland soil is the best adapted to its wants; it might thrive much better in rich, deep, loamy soils—only there it encounters dangerous competitors, which in a thin moorland soil cannot exist at all.

Now if we, bearing these considerations in mind, enquire again what is the "natural food" of man, we shall be forced to admit that he is semi-carnivorous. As a rule he prefers a diet consisting, in part at least, of the flesh of animals. This craving for flesh is not a matter of climate; it is experienced in equatorial regions as decidedly as in semi-arctic Britain, or even in polar lands.* Nay, where animal food is scarce, there cannibalism is most general. It is true that ancient traditions seem to point to a time when man, like the other anthropoids, was purely frugivorous; but we have to do not with pre-historic man, but with man as at present constituted and situated: what he was in earlier stages of his development may be a matter of speculative interest, but can throw no light upon the diet he ought to adopt to-day.

Again, it is urged that we have a natural repugnance to animal food which is only rendered palatable by the artificial processes of cookery: this, also, is an error. The Tartar, when setting out on a journey, will place a piece of raw meat beneath his saddle, and, after a ride of some four or five hours, consume it eagerly, with no other preparation. The German will eat raw ham, *Trichinæ* included, with evident relish. A very large proportion of the meat consumed

* Bates, Naturalist on the Amazon, vol. ii., p. 214.

in England—at eating-houses, hotels, and even in private families—is called "cooked" by an extravagant figure of speech. Raw oysters are never objected to by the daintiest epicure. Nor must it be forgotten, on the other hand, that much of our vegetable food is only rendered fit for use by the arts of the cook. Of the two evils, we should certainly prefer a raw beef-steak to a raw potato, and should find it much more digestible.

We are further reminded that animal food exposes its consumers to a variety of dangers. The ox or the sheep may have been suffering from "rinder-pest," or pleuro-pneumonia; the pork may be trichinised. Flesh of any kind may be passing into decomposition. Sausages may convey the germs of that ill-understood poison which has proved fatal to so many lovers of such questionable dainties. Mussels may, as it seems, capriciously poison one guest and leave ten others unhurt. Poisonous fishes exist in numbers, and we are as yet far from an exact knowledge of the dangerous species. We admit all this to the fullest extent; but we reply that if the path of the flesh-eater is beset with snares, that of the vegetarian is undermined with pitfalls. Putrescent fruits and vegetables are no less common than tainted meat and game, and certainly not less injurious to the eater. Fruits and salads may introduce Entozoa into the system as decidedly as raw ham. Vegetables have their diseases, under whose influence they become decidedly unwholesome. The fungus tribe, like the fishes and Mollusca, comprise both salutary and poisonous species, and exhibit much of the same capriciousness in their action which characterises the latter. Nor must we forget that numbers of persons have perished from mistaking some deadly herb, root, or fruit for an esculent species. Fool's-parsley has been confounded with parsley, the roots of monk's-hood with horse-radish, and the very berries of deadly nightshade have been made into pies! These facts prove not that we should abandon either vegetable or animal substances as a class, but that in the selection of both we should be prudent and wary. To eat raw ham, sausages of unknown origin and history, fishes of doubtful species, and the like, is certainly "tempting Providence." But equally foolhardy is the man who eats fruits "on the turn," fungi gathered by inexperienced persons, or salads and celery from a sewage-irrigation farm.

The teeth and the digestive organs of man, it is said, are indeed in their structure intermediate between those of the Carnivora and of the Herbivora. But this, it seems, proves

not that we are adapted for a mixed diet, but that our "natural food" is something holding a middle place between animal and vegetable substances. Such substances, we are further told, are fruits, roots, and seeds—bodies, we must observe, all essentially vegetable, and widely differing in their chemical composition. It is further asserted that as our nearest zoological affinities, the apes, are vegetarians, our flesh-eating habits must be a departure from what Nature enjoins.

In reply to all this we must point out that the organisation of an animal throws a far less clear light on its habits than was supposed in the days of Cuvier. Nor is the matter much more conclusively settled by zoological affinities. Among the bears, for instance, we find the polar bear as purely carnivorous as the lion or tiger. The dreaded grizzly of western North America is scarcely, if at all, less exclusively a flesh-eater. But in tropical and sub-tropical climates we find several bears which live chiefly upon roots, grain, fruits, and honey, and rarely, if ever, consume animal food, except they can find no other. Now between these bears, differing thus widely in their respective diet, we find no well-marked distinction in the structure and length of the intestinal canal, or in the nature of the teeth. Again, among the rodents, we find the hare and the rabbit purely herbivorous, whilst the mouse and the rat are omnivorous—not merely devouring dead animal matter when it falls in their way, but sometimes attacking and killing living animals.* Now the structure of the rat and the mouse certainly gives but very slender, if any, reason to suppose that they are better adapted for an animal diet than the hare or the rabbit. In view of such facts we submit there is nothing either in man's dentition and the structure of his intestinal canal, or in his morphological affinity with the gorilla and the chimpanzee, to prove him unfitted for the use of animal food. He certainly approaches as nearly to the carnivorous type as does the mouse, the rat, or the swine, which latter animal—even in a wild state—is semi-carnivorous. Surely, therefore, all vegetarian arguments

* That rats will kill and devour guinea-pigs, chickens, cage-birds, &c., is a matter of common notoriety. As regards mice, their predatory habits are less generally known. The present writer, many years ago, caught a field-mouse, and placed it in a collecting-box to carry home as a delicacy for some captive vipers. In the same box were three lizards (*Lacerta crocea*). On arriving at home we found the lizards all dead—each killed by a bite on the throat. It is quite possible that they may, in like manner, overpower their great enemies the viper and the Austrian adder (*Coluber austriacus*), when the latter are benumbed with the winter's cold. Mice, likewise, wage war with scorpions, with varying success.

drawn from anatomy and zoology may be dismissed as fallacious.

Perhaps the only remaining argument of the vegetarian party which still requires to be discussed is the assertion that our present system of diet involves cruelty to animals. The question resolves itself into this—Have we the right to take animal life at all? or, to put the matter more plainly, have we the right to exist? The alternative is put before us, to kill or to be killed. We can scarcely move without destroying some minute organism. We are compelled to extirpate, as far as practicable, beasts of prey, venomous reptiles, parasitic vermin, and Entozoa. In our agricultural operations, by digging, ploughing, liming, and draining the soil, we slay unwittingly and incidentally, but not the less surely, legions of worms and larvæ. Not only so, but we are obliged, for the defence of our crops, to wage a constant war against field-mice, rats, hamsters, graminivorous and fruit-eating birds, caterpillars, sawflies, aphides, locusts, earwigs, mole-crickets, weevils, wireworms, slugs, and other animals down to the *Phylloxera* and the *Oidium*. Vegetarianism, therefore, would not exempt us from the necessity of destroying animals to secure, if not to procure, our sustenance. Nay, it is very probable that the numerical amount of life taken directly for our animal diet would sink into utter insignificance compared with the slaughter necessary in the cultivation of our vegetable food. Now, if we are justified in thus killing to defend our crops, does it not seem very hazardous to contend that we may not kill for the direct purpose of eating? True, the enemies of our fields and gardens are generally of a small size and a low organisation. But it would be absurd to permit the death of a locust, and condemn the slaughter of a sheep.

It must further be remarked that those animals which we now rear for food would, if no longer required, be infallibly doomed to extirpation. The hog, the hare, the rabbit, the turkey, duck, goose, &c., could not be permitted to go on increasing in our midst, and consuming the produce of the soil, when no longer able to make us any return in their flesh. The future even of the ox and the sheep would become problematical. It may be very well for Shelley to sing—

> "The dwellers of the earth and air
> Shall crowd around our feet in gladness,
> Seeking their food or refuge there."

But in sober prose the disuse of animal food will mean to many species not emancipation, but destruction. Hence it

appears that vegetarianism, so far from putting an end to the havoc which we now make among the lower animals, would leave the greater portion of it untouched, and would rather lessen than increase the amount of animal enjoyment on the earth.

Summing up the total result of our enquiries, we find that the abolition of our fisheries and the disuse of our pasture-lands must render the promised economical advantages of vegetarianism highly problematical. We see no good reason to expect either moral or physical benefit, to any marked degree, from the dietetic reform. We see that the strictest vegetarianism would not exempt us from the wholesale destruction of animal life. Nor can we find any soundness in the lines of argument hitherto employed to show that the flesh of animals is not a fit food for man.

Until some totally different evidence shall have been adduced we cannot consider our present mixed diet one of the causes of the woes that afflict humanity.

Though unable to accept the theories of vegetarians, it must not be supposed that we entertain any hostility to their practice. If any man finds that a purely vegetable diet is better suited to his health, his sentiments, or even to his pocket than a mixed regimen, we do not for a moment contest his right to act upon the results of his experience. Nor can we demur to any temperate and rational attempts at bringing over others to the same way of thinking; but unfortunately every social "movement" takes the earliest opportunity to constitute itself an intolerance, and to become offensive in every sense of the word. To this rule vegetarianism forms no exception; its advocates are prone to question the sincerity of all who reject their arguments, and to accuse them of wilfully and deliberately shutting their eyes to the truth. Certain ugly names, such as "blood-eater,"* are also hurled at the heads of persons who still feel themselves justified in adhering to the roast beef of Old England. Should the party gain sufficient strength,

* A flesh-eater is not necessarily a blood-eater. The unwholesomeness of blood does not detract from the wholesomeness of meat any more than the dangerous nature of the sap of the cassava root proves its insoluble portion to be improper for food. It is too often forgotten that the blood, though conveying nutrition to all parts of the body, is also charged with the *effete* matter thrown off by the tissues on its way to the organs of excretion. It is, therefore, impossible to take blood from an animal without obtaining a mixture of sound matter with that which is utterly unfit for food. It is a humiliating fact that in a certain English seaport the blood of the cattle there slaughtered, instead of being utilised by mordant-makers and manufacturers of chemical manures, is actually consumed as food by the unfortunate inmates of the workhouse!

we should not feel at all surprised if the attempt were made to suppress flesh-eating by the power of the law. If we might presume to advise, we should suggest to vegetarians the propriety of being a little less selfish, and not seeking to force their good things upon a reluctant public. Let them remember that virtue itself is not exempt from the laws of supply and demand. If the vegetarian system possesses half the merits claimed by its advocates, those who carry it out in practice will soon evince such an unmistakable superiority to their neighbours that imputations, abusive epithets, and all the stock in trade of modern professional philanthropy will be utterly needless.

III. RECENT CHEMICAL RESEARCHES.

By M. M. Pattison Muir, F.C.S.

IN the following paper we propose to give an account of some of the most important recent researches bearing upon points of theoretical interest in Chemical Science.

I. *Dissociation of Elements.*

From the time of Prout to the present day the theory that the so-called elementary bodies are really compound substances has found supporters. Mr. J. Norman Lockyer has lately put forward an hypothesis, concerning the dissociation of elementary atoms by the intense heat of the sun and certain stars, which well merits the attention of chemists.

Inasmuch as all solids give continuous spectra, while all vapours produced by the high-tension spark give line-spectra,—inasmuch, further, as compounds give continuous spectra, but are resolved by the high-tension spark into their constituent elements,—the conclusion may be drawn that an element in a solid state is more complex than the same element in a state of vapour.

The following five stages in spectrum complexity are distinguished:—

First stage of complexity ..	Line-spectrum.
Second ,, ,, ..	Channelled space-spectrum.
Third ,, ,, ..	Continuous absorption at the blue end.
Fourth ,, ,, ..	Continuous absorption at the red end.
Fifth ,, ,, ..	Unique continuous absorption.

These different spectra are believed to be due to different molecular aggregations. The spectra of the non-metallic elements do not exhibit the same order of change, with

increase of temperature, as those of the metallic elements. Sodium exhibits a line spectrum at a dull red-heat; at the same temperature the spectrum of sulphur is characterised by a continual absorption in the blue: as the temperature increases the line spectrum of sodium remains unchanged, but the spectrum of sulphur gives place to a channelled space spectrum, which remains until the temperature of the electric arc is reached, when a line spectrum makes its appearance. There are therefore three stages of non-metallic spectra corresponding to three stages of heat, but these three stages do not correspond with the stages of the spectra of metallic elements: hence the inner structure of the non-metals probably differs from the inner structure of the metals.

The spectra of the substances which are present in the reversing layer of the sun are all line spectra; hence it is inferred that these substances are all in a similar state of aggregation. But we saw that at comparatively low temperatures non-metals are characterised by channelled space spectra, which are believed to indicate a more complex inner structure than is shadowed forth by line spectra. The inference to be drawn from these facts (admitting the connection between simple spectra and simple molecular constitution) seems to be that the formation of those molecular aggregations which are presented to us by the non-metallic elementary bodies on the earth is not possible at the temperature of the sun.

This hypothesis is put forward by Mr. Lockyer merely as a means for guiding future work: from the chemical view point it is exceedingly interesting. That the non-metallic molecules should be more ready to undergo change than the metallic molecules is in keeping with many known chemical facts. The phenomenon of allotropy occurs to a considerable extent among non-metals, and it is almost certain that allotropic changes are always accompanied by variations in molecular aggregation. The change of ordinary into amorphous phosphorus, and *vice versa*, would seem to depend essentially upon the tension of the phosphorus vapour; and what does this statement imply but that, under certain conditions, the molecules of phosphorus are so packed together as to exhibit to our senses the phenomena associated with what we call ordinary phosphorus, while under other circumstances the state of molecular aggregation is such as that the body exhibits the phenomena associated with amorphous phosphorus?

Mr. Lockyer defines a metal, provisionally, as a substance

whose absorption and radiation spectra are the same, and non-metals as substances in which these two spectra differ.

Furthermore, these researches introduce a new definition of the "atom" as being that mass of matter which gives us a line spectrum, while the mass of matter (non-metallic) which gives us a channelled space spectrum is called a "sub-atom."

The plasticity of non-metallic molecules is a subject to which we shall have occasion to make further reference.

II. *Atomic Weights of the Elements.*

Of the various methods which may be employed for ascertaining the atomic weight of any element, that which is based upon the determination of the density of the element when in a state of vapour and the densities of the greatest possible number of gaseous compounds of that element holds the first place. By checking the results of such vapour density determinations by means of gravimetric analyses we are able to fix on the maximum atomic weight of the element. As a means of determining the atomic weight of those elements which do not assume the gaseous condition at any attainable temperature, the method of specific heat has generally been adopted. This method is based upon the fact that the average product obtained by multiplying atomic weight into specific heat (called atomic heat) is, in the case of the solid elements, represented by the number 6·3. The fact that the atomic heats of the three solid elements, silicon, boron, and carbon, are represented by the numbers 4·8, 2·7, and 1·8 respectively, has rendered the application of the method of specific heats to atomic weight determinations somewhat unreliable. If these three elements deviate so widely from the general rule, what guarantee have we for believing that a similar deviation does not occur in the case of other elements the determination of the atomic weight of which is as yet an unsolved problem?

Prof. Weber, of Hohenheim, has made a number of exceedingly accurate determinations of the specific heats of carbon, boron, and silicon, from which he arrives at the result that these elements obey the law of Dulong and Petit. On comparing the numbers given by different observers for the specific heats of the three bodies in question, it was ascertained that these numbers represented the specific heats for different intervals of temperature, and that the greater the interval of temperature for which the determination was made the greater was the number representing the specific heat.

In a preliminary series of experiments Prof. Weber showed that the specific heat of diamond increases, with increase of temperature, more quickly than that of any other substance—the values at 0°, 100°, and 200°, being almost in the ratios 1 : 2 : 3. Hence it follows that there is a close relation between the specific heat of diamond and the temperature, and that these quantities increase simultaneously. The value of this increase, however, diminishes at a red-heat and upwards to a white-heat, until it becomes but a seventeenth part of what it was for the temperature-interval 0° to 100°.

From a red-heat upwards the value of this increase is not greater than that of the increase noticed in the case of the elements which obey the law of Dulong and Petit.

The actual numbers representing the specific heat of diamond at high temperatures are as follows:—

Temp.	Spec. Heat.
606° C.	0·4408
806°	0·4489
985°	0·4589

By multiplying these numbers by the generally received atomic weight of carbon we obtain a product varying from 5·2 to 5·5. Other elements which have small atomic weights give very similar numbers: thus the product obtained by multiplying the atomic weight of aluminium into its specific heat is 5·7; of phosphorus, 5·5; of sulphur, 5·5; &c. Crystallised boron shows a like increase in specific heat with increasing temperature; at —39·6° the number expressing the specific heat is 0·1915, while at 233·2° it is 0·3663. By reasoning from these numbers the conclusion is drawn that at a red-heat the specific heat of boron attains a constant value equal to about 0·5. If this number be multiplied by 11, the generally-received atomic weight of boron, we obtain the product 5·5, which expresses the atomic heat of this element.

Similar researches, carried out with crystallised silicon, have led to similar results, viz., that the specific heat of silicon is a function of the temperature, and that at about 200° it attains a constant value: 0·2029 is the experimental number representing the specific heat of this element at the temperature 232·4°; 28, which is the received atomic weight of silicon, multiplied into 0·2029 gives us the product 5·68.

The three elements, carbon, boron, and silicon, are therefore no longer to be considered exceptions to the generalisation of Dulong and Petit; and we may, with more certainty

than heretofore, apply this generalisation in the determination of disputed atomic weights. A modification must, however, be made in the statement of the law, thus:—"The specific heats of the solid elements vary with the temperature: for every element, however, there is a point (T_0) from which the variation in the specific heat, with increasing temperature, is entirely insignificant. The product of the atomic weight into the value of the specific heat (estimated at temperatures so that $T > T_0$) is, for all the elements, a nearly constant number varying from 5·5 to 6·5."

The bearing of Prof. Weber's researches upon the specific heat of allotropic modifications of the same element, and also on the question of varying chemical value of the same element in its compounds, will be discussed hereafter.

Changes have been proposed by Mendelejeff in the numbers which express the atomic weights of yttrium and erbium, of cerium, lanthanum, and didymium: these proposed changes are based upon considerations of the relations which appear to exist between the atomic weights and the properties of the elements. These relations exhibit the form of a periodic function. If the elements be arranged in order of their atomic weights, we may divide them into groups the properties of the members of each of which exhibit a regular variation as the atomic weights increase. Now, if the atomic weights generally accepted for yttrium, erbium, cerium, lanthanum, and didymium be the true atomic weights of these elements, we find that these bodies do not occupy the positions in the groups which, judging from their properties, we should expect they would do. But as we are certain, so far as investigation has gone, of many of the properties of those elements, while we are not at all certain of their atomic weights, Mendelejeff proposes to alter the latter, so that the elements may fit into those places which, according to his theory, they ought to occupy.

In order that the atomic weights of these elements should be more definitely settled it is necessary that the specific heats be determined. Mendelejeff states that he has determined the specific heat of cerium to be 0·05: this number multiplied into 138 (the number proposed for the atomic weight of this body) gives 6·9, which is in keeping with the average atomic heat of the elements. Nilsson is at present engaged in the preparation of considerable quantities of these metals, with the view of determining their specific heats.

III. *Classification of the Elements.*

We have already referred to Mendelejeff's system of classification according to atomic weights: the more commonly employed systems of classification are based upon the chemical value or valency of the individual elements. The researches of Michaelis, of Meyer, and of others, have important bearings upon the general question of valency.

That the valency of each chemical element is invariable throughout the whole of its compounds has been and is maintained by many of our most distinguished chemists: the doctrine opposed to this—viz., that the valency is a varying quantity—has also been upheld by able supporters. Before we are in a position to discuss this question it would be well that we should have a clear idea of what chemical valency means. Adopting the usually-received definition of "atom" and "molecule," we say that one atom of carbon is capable of combining with four atoms of hydrogen, while one atom of oxygen is capable of combining with only two atoms of hydrogen. The carbon atom is therefore said to be "equivalent to" four hydrogen atoms. But we must remember, as Mills has pointed out, that carbon and hydrogen atoms have never been compared as to the work they can do under certain circumstances, and that the relations of their potential energies have yet to be determined. We are very apt to confuse two things which are, most probably, totally distinct—affinity and chemical valency.

Michaelis imagines a carbon atom performing vibrations so that there are four positions during each complete vibration in which it is possible for the atom to come within the sphere of action of another atom: he imagines an oxygen atom performing vibrations so that there are but two positions during each complete vibration in which it may come within the sphere of action of another atom. Or we may imagine the carbon atom as exercising force in four different directions, while the oxygen atom exercises force in but two directions. The number of positions of advantage, as we might call them, or of lines of force, may then be independent of the total force exercised. We may imagine an atom exercising force in but two directions, and nevertheless exercising an absolutely greater *amount* of force than another atom the directions of the exercise of whose force amount to four in number. The total force exercised is therefore the affinity, and is dependent upon the nature, position, &c., of all the atoms in the compound: the number of directions,

or the number of positions, in which this force may be exhibited is the chemical valency of the atom.

Now it is not necessary to suppose that the total force is exercised equally in each of the various directions; it is possible that a greater amount may be exercised in direction 1 than in direction 2: hence there may be a difference in the nature of a compound according to the direction in space of the lines joining the centres of the component atoms.

The mutual action of atoms is supposed by Michaelis to be for certain distances an attractive action, but for very small distances a repulsive action. Hence when two atoms come within the sphere of one another's action they approach one another until the repulsive action comes into play, when they are repelled until the repulsion gives place to attraction, and so on. But under certain circumstances, which may easily be imagined, the atoms are driven so far asunder that return within the sphere of one another's action is no longer possible; the compound, therefore, is broken up.

In this view chemical valency is constant for each element, inasmuch as the number of directions in which the chemical force may be exerted is a constant number; but the force need not be equal in amount in each direction, nor need it be always expended in doing work in each direction when the element unites with others to form a compound substance. Furthermore, the valency is independent of the total affinity of the element, so that when we speak of tetravalent carbon and divalent oxygen we do not mean it to be inferred that the potential energy of the carbon atom is double that of the oxygen atom, but merely that the carbon atom is able to exercise its total energy in twice as many directions as the oxygen atom is able to do.

The chemical valency of the elements may therefore form the basis of a system for their classification. In order to determine the valency of any element it is necessary to take into account the whole of its compounds so far as we are acquainted with them. But inasmuch as we cannot determine with certainty the molecular weights of any compounds other than those which are gaseous at attainable temperatures, we must limit our attention to such compounds. Conclusions concerning valency, drawn from a consideration of solid or liquid compounds, are very misleading.

The existence of so-called molecular compounds is liable to mislead in the determination of the valency of an element. No definition, which may be easily and exactly applied, of a molecular compound as distinguished from an atomic

compound, has yet been given. Michaelis proposes to adopt the rule that when a doubtful compound yields an atomic compound by double decomposition it is itself an atomic compound. Inasmuch as ammonium chloride yields ammonia (the molecule of which is represented by the formula NH_3) by the action upon it of lime, we should probably be obliged to class this compound among those which are atomic. But we know that the action of heat upon ammonium chloride resolves it into ammonia and hydrochloric acid; is it not, then, very probable that the seeming double decomposition which takes place between sal-ammoniac and lime is in reality an action between hydrochloric acid and lime, and that the first action of the heat is to decompose the ammonium chloride with the production of ammonia, which escapes, and hydrochloric acid which acts upon the lime? In the case of such compounds as ammonium chloride it is very difficult to determine whether cases of seeming double decomposition really belong to this category or not.

But if we agree with Michaelis's general view of valency —viz., that it is measured by the number of directions in which the energy of a chemical element is exercised, but that the amount of energy need not be the same in each direction—we may imagine that the weaker lines of force are those which are concerned in the formation of the so-called molecular compounds. Let us take the case of sal-ammoniac. Ammonia gas and hydrochloric acid gas, when brought together in proper proportions, are no longer to be distinguished as such; their properties become merged in those of the new solid body formed. But if this new body be heated it is resolved into ammonia and hydrochloric acid; if the mixture of heated gases be allowed to cool the solid body is re-formed. Let us suppose that there are five positions in which the nitrogen atom can exercise its chemical energy during each complete vibration. In ammonia the nitrogen atom is brought within the sphere of action of three hydrogen atoms, and we may suppose that during each complete vibration there are three positions at which the attractive force of each hydrogen atom in turn changes to a repulsive force; the nitrogen atom therefore oscillates about these three positions, and at the same time about two other positions at which it is possible for other atoms to exercise an action upon it. And it is at these two latter positions that the action of the hydrochloric acid molecule is exhibited. But inasmuch as we know that the total energy of chlorine is large, although it be exerted in but one direction, while

the total energy of nitrogen is comparatively small, it seems difficult to imagine that the hydrochloric acid molecule will be split up into its constituent atoms, and that these will then each oscillate about the nitrogen atom. But is it not possible to avoid this difficulty by supposing that there is such a thing as molecular as well as atomic valency?—that although in the hydrochloric acid molecule the lines of force of its atom are mutually satisfied, there may be directions in which the molecule, as such, may exert a certain amount of chemical energy? On this view we may picture to ourselves the hydrochloric acid molecule oscillating about these two points, at which the nitrogen atom is still free to exercise chemical force. But the force exercised at these points is, by supposition, less than that exercised at the other three points; therefore when the compound is heated the hydrochloric acid molecule is easily driven beyond the sphere of action of the nitrogen atom, and we have the phenomenon of dissociation.

Whatever theory may be adopted of actions such as this, it is evident that, in the determination of the valency of an element, attention should be paid to the whole of its compounds, but that reliable conclusions are only to be drawn from those which can exist in the gaseous form.

Researches bearing upon the valency of the individual elements have been carried out of late by various observers. Many disputes have arisen as to the valency of the elements which constitute the nitrogen group, so that an especial amount of interest attaches itself to any new researches into the constitution of the compounds of the elements. Michaelis concludes, from general observation of a large number of its compounds, that phosphorus is a pentavalent element. Wurtz has determined anew the vapour density of phosphorus pentachloride, by diffusing the vapour of this compound into a space filled with the vapour of phosphorus trichloride. By this means the dissociation of the pentachloride was prevented. The number 7·226 represents the average vapour density calculated from the results of twelve experiments; the theoretical vapour density for PCl_5 (two volumes) is 7·217. We have in this determination an almost certain proof of the pentavalency of phosphorus. Another proof that phosphorus is really a pentavalent element has been supplied by Thorpe, who has succeeded in preparing a gaseous pentafluoride of phosphorus, PF_5, by the action of arsenic trifluoride upon phosphorus pentachloride: the new compound acts upon glass, and is soluble in water, but may be collected over mercury. It seems, then, very probable

that the elements of the nitrogen group are really pentavalent.

Exceedingly interesting results are certain to follow from researches into the total affinity force exercised by the elements as distinguished from their chemical valency. Hitherto this field of enquiry has had but few cultivators, probably on account of the difficulties which beset the problems presented. A step in advance has been made by Wright, who has calculated the affinity of the elements, in several compounds, in terms of the heat gained or lost during the formation of the compound from its constituent elements; but as Wright's determinations have a more immediate bearing upon the questions to be considered when speaking of compound bodies, we shall defer a more detailed account of them to a later part of this paper.

It is well known that several of the elements can exist in various so-called allotropic forms: these forms seem to be connected with the original form of the element in somewhat the same way as isomeric compounds are connected with the primary forms from which they are derived.

We have already seen how Lockyer's spectrum researches lead to the idea that the molecules of the non-metallic elements are possessed of a large amount of plasticity: the facts noticed in the study of the chemical history of these molecules fully confirm this theoretical deduction. If oxygen be subjected to the action of the silent discharge it exhibits many new properties, yet we know that from the substance thus formed, oxygen—and oxygen only—can be obtained. We are obliged, therefore, to imagine that a change has taken place in some way in the inner structure of the oxygen molecule, or that under the altered circumstances that molecule is endowed with different potential energy from that which it formerly possessed. The formation of one or other of these molecules is conditioned by the temperature; at a temperature of 300° C. ozone is decomposed with the formation of common oxygen. As the general action of heat is to cause greater freedom of molecular motion, we may perhaps conclude that the atoms in the molecule of oxygen are possessed of a greater freedom of motion than those in the molecule of ozone. So, also, we know that if ordinary phosphorus be heated to a certain point, in an atmosphere incapable of acting upon it chemically, amorphous phosphorus is produced; but if the temperature be increased the production of amorphous phosphorus ceases, and that which is formed is re-transformed into the ordinary variety. This action is clearly conditioned by the temperature, and therefore by the

tension of the phosphorus vapour. It is found that for a given temperature there is a certain fixed tension of the vapour required to hinder the transformation of ordinary into amorphous phosphorus. As the temperature increases, however, the transformation is again set going, until the tension of the vapour increases to such an extent as again to stop the action. If phosphorus be heated in a confined space, different parts of which are maintained at different temperatures, the tension of the vapour will prevent the formation of the amorphous variety, except at the hottest parts of the space. May we imagine that some such state of affairs prevails in the atmosphere of the sun and of the hot stars?—that the atomic alteration, shadowed forth by Mr. Lockyer's researches, which appears to take place in non-metallic molecules, consists not in the decomposition of these molecules with the production of simpler forms of matter, but in the production of isomeric or allotropic forms of them with which we, on the Earth, are as yet unacquainted? —that at ordinary temperatures the tension of the vapours of the elements is sufficient to stop this action, but that at the high temperature of the sun's atmosphere the transformation takes place?—and that, as these allotropic forms ascend to cooler parts of the sun's atmosphere, the tension of their vapour is able to bring about the re-formation of those molecules which are known to us on the earth?

That quantities of energy are absorbed or evolved in the passage of one allotropic form of an element to another is evident from the results of experiments made by Mitscherlich and others upon the forms of sulphur. The conversion of 1 grm. of dissolved octahedral sulphur into insoluble amorphous sulphur is attended with the evolution of 12,800 heat-units. A careful study of the loss or gain of energy, in the reciprocal formation of allotropic varieties of the non-metallic elements, would doubtless lead to most important results.

Weber has found that the specific heats of the various allotropic forms of carbon differ at low temperatures, but that at those temperatures at which the optical differences begin to disappear the differences in specific heats disappear also. At high temperatures carbon has but one specific heat; hence it exists apparently in but one form.

The facts about allotropic modifications of elementary bodies may possibly have an influence upon the constitution of compounds. Weber concludes from researches, a full account of which has not yet been published, that the specific heat of carbon in combination varies according to the

nature of the non-carbonated portion of the compound molecule: he professes himself unable to explain such variations, except on the assumption that the carbon atom is possessed of great plasticity, and that it exists in combination in different allotropic forms. Gautier has also obtained compounds of phosphorus in which that element appears to exist in the amorphous condition; while Houzeau and Renard, by the action of ozone upon benzene, have produced a peculiar body which appears to contain oxygen in its allotropic form of ozone. We may therefore suppose that in the formation of isomeric compounds a transformation of one allotropic form of the dominant atom into another takes place, and that—inasmuch as such a transformation is accompanied with changes in the potential and kinetic energy of the atom—the compounds will differ from one another in the amounts of energy which they contain, although the valency of the individual atoms may be the same in all.

But considerations such as these lead us to speak of—

IV. *Groups of Compounds.*

And here we are, of course, met by the great fact of the existence of isomeric compounds. We must regard the molecules of all substances as material systems. Now Prof. Clerk-Maxwell has said that when we can explain any phenomena as changes in the configuration and motion of a material system, we have given a complete dynamical explanation of these phenomena. We must therefore endeavour to explain the phenomena of isomerism as changes in the configuration or motion of the molecules of the isomeric bodies. The ordinary way of representing such changes is by placing the symbols which represent the atoms of the molecule in different relative positions in space in the different formulæ. Isomerism in this view is caused by a change in the position of the atoms. But we must remember that no formula can give us a true insight into the constitution of the molecule; the atoms are in a state of motion, each performing its own definite oscillation: our formulæ represent them as at rest. Another explanation of isomerism has been sought for by supposing that different amounts of energy are concerned in the production of isomeric bodies, and that these bodies therefore differ in the potential energy which they contain. Now if a greater amount of energy has been expended in the formation of the body A than in the formation of its isomer B, what has that energy been employed in doing? Surely in altering the relative motions and configurations of the atoms. Therefore

both views are perhaps most fittingly presented to us in the formulæ which are in common use. If the motions and configurations of the atoms have been altered (as our ordinary formulæ say they have) energy must have been expended in bringing about this alteration, and therefore these formulæ indirectly express this fact. Various physical considerations tell us that there is a difference in the inner constitution of isomeric bodies; their boiling-points are different, and Thorpe has shown—in a preliminary paper read to the British Association at Belfast—that their specific volumes also vary.

The propriety of attempting to express the constitution of such a body as a molecule upon a flat surface has been questioned by Van't Hoff; but it is evident, as Lodge has shown, that "any configuration in space may be projected on a plane surface, and the only difference between such a projection and a diagram drawn directly on the plane will be—that in the latter the bands are not usually made to cross each other, whereas in the former they will be very liable to do so."

A consideration of groups of compounds leads us to speak of recent attempts to systematise inorganic chemistry.

Lothar Meyer—adopting Mendelejeff's view, that the properties of each element are expressed as a periodic function of its atomic weight when the elements are arranged in the order of their atomic weights—has endeavoured to deduce general formulæ which shall express the composition of series of salts, thus introducing the homologous and heterologous series of organic into inorganic chemistry. He has expressed the composition of a number of hydrates and other salts by the general formula $H_{2n}X_2O_{v+n}$, where X represents the atomic weight of an element, v its valency as deduced from the highest oxide, and n a whole number, which generally does not exceed 4. The following examples will illustrate the application of the formula:—

	$v=1$.	$v=2$.	$v=3$.
$n=0$	Na_2O	Mg_2O_2	Al_2O_3
	Cu_2O	Zn_2O_2	B_2O_3
$n=1$	$H_2Na_2O_2$		$H_2Al_2O_4$
	$H_2K_2O_2$		$Mg_2Al_2O_4$
	$H_2Cr_2O_2$		&c.
	&c.		
$n=2$		$H_4Mg_2O_4$	$H_2Ag_2B_2O_5$
		$H_4Ca_2O_4$	
		$H_4Ba_2O_4$	
		&c.	

The formulæ in this table do not always agree with those in general use; but it must be remembered that we are altogether in ignorance of the true molecular weights of the greater number of these compounds. Similar formulæ may be deduced to express the composition of other salts; but, as Meyer points out, we need many new researches into the too much neglected compounds of inorganic chemistry before we shall be able to introduce a complete system of classification of compounds.

With regard to the notation of compounds, we have already seen that the formulæ in general use imply that a certain amount of energy has been expended in the formation of one compound from another. Wright has studied somewhat in detail the relations which exist between affinity and structural formulæ: he has sought to measure affinity by the number of heat-units gained during the coming together of several forms of matter, so as to form a given weight of a compound body. Many interesting generalisations have been deduced, important among which are the following:—

The production of ethers and steam from alcohols is accompanied by heat absorption.

The oxidation of alcohols to acids and steam is accompanied by heat evolution.

In the substitution of the group CH_3 for the hydrogen belonging to the hydrocarbonous part of a radicle heat is evolved, while in the substitution of the same group for the hydrogen belonging to the hydroxylic (OH) part of a radicle heat is absorbed.

The replacement of H_2 by O is accompanied by heat evolution.

These results open the way to most interesting considerations regarding the work done in the formation and transformation of chemical compounds; but before any generalisations can be safely made a large amount of experimental data must be accumulated.

Among the general results which have been deduced by Wright we find the following statement:—"If an operation be performed such that heat is evolved during its performance, the resulting product boils at a higher temperature than the original substance, and *vice versa*." This statement is in accordance with the views of Mohr, and also with those promulgated some years ago by Odling. If we understand by the evolution of heat the loss of molecular motion, it follows that the resultant must be more viscous, so to speak, than the substance producing it, and must therefore boil at a higher temperature. The subject of the influence

of heat upon chemical decomposition has been studied by Mohr. In order that a chemical compound may be dissociated by heat alone, it is necessary that one or more of its constituent elements shall be gaseous at attainable temperatures. When the gaseous element or elements became combined, so as to produce a non-gaseous compound, a quantity of heat was evolved,—that is, molecular motion was arrested: the process of dissociation will therefore be accomplished if the quantity of heat lost on combination be restored, because the molecular motion of the constituents of the compound will then be so great that they will no longer hold together. If a large quantity of heat be evolved in the formation of any compound we find that it is hard, or in some instances impossible, to decompose that compound by heat alone. In the formation of carbon disulphide from carbon and sulphur heat is absorbed, because the elements are transformed from solids into liquids: now carbon disulphide may be decomposed by the heat of an ignited platinum spiral, but in this experiment heat is evolved, the amount being equal to that formerly absorbed in the liquefaction of the carbon and of the sulphur.

Looking at the recent researches in chemistry as a whole they all point to the advance of the time when chemistry shall have become a mathematical science, whose laws shall be formulated with the greatest exactness, and whose results shall allow of the application to them of mathematical reasoning.

IV. SIDEREAL ASTRONOMY: DOUBLE STARS—FAR-OFF WORLDS.

By Camille Flammarion.

I. *General Considerations.*

IN the depths of the heavens, amongst the varied stars which shed their silent light above the regions of the starlit night, the eye of the telescopic investigator discovers stars of a particular character, which differ from the ordinary stars in their appearance, as also in the part they play in the universe. Instead of being simple, like the greater majority of the stars of heaven, they are double, triple, quadruple, multiple. Instead of being white, they

often sparkle with coloured light, offering in their strange couples admirable associations of contrast, in which the astonished eye sees the fire of the emerald wedded with that of the ruby, that of the topaz with that of the sapphire, the diamond with the turquoise, or the opal with the amethyst, glittering thus with all the shades of the rainbow. Sometimes the wonderful stars which form these celestial couples repose in the vault of infinity, fixed and unchangeable; and for more than a century that astronomers have been anxiously contemplating and watching them, they have not varied their relative position with regard to each other: as the searching look of the patient William Herschel surprised them there a hundred years ago, so we find them to-day. Sometimes, on the contrary, the two associated stars gravitate round their common centre and turn round each other, the weakest round the strongest, rocking on the wing of attraction, as the moon moves round the earth and the earth round the sun. A certain number of these couples have already made many complete revolutions under the eyes of observers, the duration of these revolutions differing with each couple, and offering the greatest variety of periods, from a few years only up to thousands of years, and even to hundreds of centuries. Our small terrestrial calendar does not extend its empire to these far-off worlds; our ephemeral periods, our ant-like measurements, are strangers to these grandeurs; the earth is no longer the measure of creation, and our most sacred eras are unknown in the heavens. The study of these stellar systems constitutes one of the vastest and most interesting problems of the contemporary astronomer; it has, however, remained stationary, and, notwithstanding the considerable number of accumulated observations, no one astronomer has yet tried to gather up the harmony of all these systems, to search for differences which can exist between them, and (except some partial attempts) to catalogue on the one hand the double and multiple stars which have a certain orbital movement, and on the other hand those groups whose movement is not orbital, but rectilinear, and caused by perspective, owing to the displacement of one of the stars situated by accident before another farther off and immovable.

Each star being a gigantic sun, shining with its own light, a focus of attraction, of heat, of activity, and life, the problem presented to the human mind by these systems of multiple suns is, without doubt, one of those which can most absorb the imagination, fire the thoughts, and affect

even the heart of a philosopher. What part does gravitation play in these solar systems, so different from ours? What is the numerical importance of these systems in the sidereal world? What is their mode of distribution in the universe? What links have they to simple suns like ours? What is the nature of their light, often so strange and so fantastic? To what respective distances can the stars be associated and governed in common by a proper movement in space? What is the condition of the planetary systems which can gravitate round these double suns? What can be the physiology of these planets, governed, illuminated, heated, alternately and simultaneously, by two suns of different masses, of different distances, and different lights? And, finally, what are the wonderful and extraordinary conditions which can be brought to life on these unknown worlds, lost in the depths of the fathomless heaven? Those are the questions which have occupied my thoughts, and which I have tried to elucidate successively; such is the subject of a great work which I have ventured to attempt, and of which the extent has been much more considerable than I had at first supposed. I have been able, happily, to have in my hands nearly all the observations made on double stars since the commencement; there have been more than a hundred and fifty thousand observations, both of angles of position and of angular distances,—that is to say, more than three hundred thousand together. I have compared them all, and by these comparisons I hope to be able to determine the different species of double stars. The total number of multiple stars of all kinds discovered to this day would reach to 10,487. Of these, a very remarkable number already have been proved to form true physical systems, possessing certain orbital movement. Others remain fixed in the same position. Others are carried away into space by a common movement, and traverse immensity with a bewildering velocity. Others sparkle with a changing brightness, and we see their mysterious light sometimes diminishing and sometimes augmenting. Others even are completely extinguished. My earnest endeavour will be to sum up here in one rapid study, and in a form accessible to all clear minds, the whole of the conclusions to which these researches have conducted me; to explain these phenomena in a picture which will not be too unworthy of these wonders; and to exhibit before the eye all the power and the splendour of these far distant suns, which govern in the depths of space unknown worlds and existences.

II. *History of Double Stars.*

Double stars as such were not known before last century. However, one finds in ancient astronomical works many mentions of stars very near together, which they termed double. Ptolemy himself, in the "Almageste," describes one in τοξοτης (Sagittarius*), which he named διπλους, double. One sees it, in fact, double to the naked eye, and it is composed of two stars very near together (ν^1 and ν^2). They are of the fifth magnitude, and separated by 14 minutes of arc. The star Mizar, or ζ of the Great Bear is alike described from the greatest antiquity as double, or rather as being accompanied by a little star, visible also to the naked eye to those with good sight. The Arabs call it Saidah,—that is to say, *the test*, because they used it to test their sight. It is also named Alcor, and its companion is of the fifth magnitude, and is situated at 11 minutes of arc from Mizar, whose brightness, which is of the second magnitude, eclipses the smaller from ordinary sight. The other stars seem equally joined two and two, and appear to the naked eye as apparently double. Such, also, are α^1 and α^2 of Capricornus, which are—the former of the first magnitude, the latter of the third, and distant from each other 6′ 13″; good sights (that of the German astronomer Heis, among others) can separate them: ν^1 and ν^2 of the Crown, of fifth magnitudes, separated by 6′ 19″, a little more difficult to divide into two with the naked eye; θ^1 and θ^2 of Taurus in the Hyades (5′ 37″); π^1 and π^2 of Pegasus; δ^1 and δ^2 of the Lyre; ϵ^1 and ϵ^2 of the same constellation. Another couple could be added to the preceding; it is α^1 and α^2 of the Balance, whose separation is of 3′ 49″; but the principal star being of the third magnitude, and the second of the sixth, I doubt whether it can be divided with the naked eye, and I know nobody who can do so. Do these apparent couples constitute themselves true binary systems? This is not probable. In general double stars are incomparably closer. However, it must not be affirmed that their connection is only accidental, and due only to chance and to perspective. We shall find farther on examples of true common movements: we are justified in thinking that these stars may be associated together, although they may be more removed still from others on the celestial sphere.

The study of these double stars only dates from telescopic investigations. By a curious chance the first star separated

* In a Latin translation which I possess of the "Almageste" (Venice, 1828) this star is thus described:—"Quæ in oculo est nebulosa et bina."

by the telescope is that which was most remarkable to the naked eye, as being apparently double,—Mizar,—which is not only accompanied by Alcor, but is itself double. It is composed of two stars, separated by 14 seconds of arc from each other; the brightest is of the second magnitude, and its companion is of the third. This star was recognised as double for the first time by Riccioli, in the middle of the seventeenth century. In 1700 Gottfried Kirch and his learned wife Maria Margareta described it, and again drew the attention of astronomers to it. Bradley, Mayer, and Herschel observed it afterwards. In comparing the observations made during two centuries, we find that the two stars have hardly changed position with regard to each other. The angle increases slowly. William Herschel had concluded that it was diminishing, but this is an error of the illustrious astronomer. It is possible that the three stars (the double and Alcor) form a triple system. They are all three animated by one common movement in space.

The second star separated with the telescope is Mezartim, γ of the Whale, which was separated by Hook when observing the comet of 1664. The principal star is of the $4\frac{1}{2}$ magnitude, the other of the fifth; their distance apart is 9 seconds. Their relative position has not varied since the earliest observations.

The third star separated is that which has since become one of the most interesting, on account of its parallax—our neighbour α Centauri. It was described in 1709 by Feuillée, at Lima. Its two components turn rapidly round each other, following a much elongated ellipse, half the principal axis of which measures 14″. Since the time of its discovery there have been more than two revolutions accomplished. We give farther on the elements of this magnificent orbital system, of which the distance from the earth is eight trillions three hundred and seventy-six milliards of leagues.

In proportion as the attention of astronomers has developed and has carried them nearer to the stars, till then unknown and looked upon as simple points of light, they commenced to discover these stellar groups which had been so long unexplained. The double star γ Virginis was separated in 1718 by Bradley; Castor, α Gemini, in 1719; 61 Cygni in 1753, β Cygni in 1755. Then followed γ Andromedæ, ε Lyræ, 70 p. Ophiuchi, ζ of Cancer, β of the Scorpion, θ and ζ of Orion, ξ of the Great Bear, &c. Their numbers gradually augmented up to the time of Flamsteed, who employed a rudimentary micrometer, until the

time of Mayer, who formed a first catalogue of the double stars published in 1756. The philosophical astronomer Lambert, without having himself observed the double stars, published—in his "Photometry" (1760), and in his "Cosmological Letters" (1761)—the first exact notions on the relations of mutual attraction which ought to exist between the components of these partial systems; Lambert thought, with Kepler, that the distant suns ought to be surrounded—like our own sun—with a retinue of obscure stars similar to our planets and comets. As to the stars which were very near to one another, he believed—whilst favouring the idea of an obscure central body—that these stars ought to revolve round their common centre of gravity, and to accomplish their revolution in a sufficiently short space of time. John Michell, who did not know the ideas started by Kant and by Lambert, followed another course (1767). He applied the calculus of probabilities to the study of the stellar groups, and, above all, to the multiple stars. He proved that there were 500,000 chances to 1 that the union of these six principal stars of Pleiades could only be the effect of accident, and that some one cause had been instrumental in determining their approach. He himself became so persuaded of the existence of stars turning round each other, that he proposed the study of these partial systems as a means of solving certain astronomical problems.

Christian Mayer, an astronomer of Manheim, has the great merit of having been the first who seriously observed the double stars (in 1778). This most important branch of his work was not acknowledged until some time after his death.

These trifling but memorable beginnings were followed by the gigantic work of William Herschel, comprehending the long period of more than twenty-five years. The double nature of no star had actually been proved when William Herschel commenced his observations. The discovery of the true double stars is owing to his ingenious perseverance—above all to his observations of the beautiful double star Castor during a period of twenty-five years (1778 to 1803), showing that the relative positions of the two components varied, and that this variation had no relation with the annual translation of the earth. In following this study he was not long in proving many cases of really binary stars, groups in which the little star moved round the larger. "Thus was proclaimed," said Humboldt, "this marvellous truth, that two luminous suns by themselves—and doubtless accompanied each with a planetary system—could turn

round their common centre of gravity under the action of the same dynamic laws which govern our solar system." This, then, is a great fact, of which in all probability Newton himself had never thought.

The total number of the systems observed by W. Herschel is 812. His general catalogue was re-edited in 1867, by his son.

These researches were followed, a short time after the death of William Herschel, by observations made by his son, Sir John Herschel, and by his friend James South. They observed together in England, and afterwards separately, James South having come to Paris, and having there fixed his observatory. The first catalogue of these two astronomers contained 360 stars; the second, of South alone, contained 838. These observations were published in the "Philosophical Transactions" for the year 1826.

William Struve afterwards undertook, in Russia, his long and laborious revision of the heavens, for the verification and discovery of all the double stars. His work is composed principally of three folio volumes, written in Latin:—

1. "Catalogus Stellarum Duplicium et Multiplicium Dorpati Dedectarum. Anno 1827 editus."

2. "Mensuræ Micrometricæ Stellarum Duplicium. Anno 1837 editæ, cui accedit additamentum,"

3. "Positiones Mediæ Stellarum Fixarum Duplicium et Multiplicium. St. Petersbourg, 1852."

In making this general review of the heavens, in carrying on his investigations with the stars of the first eight magnitudes, and even on the most brilliant of the ninth, which are comprised between the North Pole and 15° south of the Equator, William Struve observed and catalogued (including Herschel's stars) 3057 double stars.

The celebrated Russian astronomer divided these stars into eight classes, in the order of angular distance, from 0″ to 32″. This division is, without doubt, very useful, but it has a defective side, which will cause it to be inevitably abandoned. All classification ought to be made on an invariable base. Nothing is more variable than the angular distance of the components of a double star. Sometimes for the same binary system this distance is less than a second, sometimes it gets beyond a second, sometimes again it gets beyond two or more. Also a star placed in one year in one class will be found in another year belonging to another class. It will be necessary to found this arrangement on mean distances; but these distances

are far from accurate determinations. As a methodical division to determine generally the number and separation of systems, the preceding is not valueless, but it is defective as the basis of a catalogue; also there is already difficulty in finding the stars when they are looked for. Thus, for example, the binary system of γ Virginis is not placed in the first class of Struve, because in 1836 the angular distance of its components was inferior to 1″, and it is found in the catalogue of P. Secchi (1860), p. 41, amongst the stars of from 0″ to 1″, although its distance in 1860 was already above 3″. At the present time, in 1875, it is 4·8″. We should thus have to place it, in 1830 in the second class, in 1836 in the first, in 1842 again in the second, in 1850 in the third, in 1870 in the fourth. Many other groups would offer analogous examples. As a system of cataloguing, one by right ascension, or even by constellation, would be evidently preferable.

After the observations of William Struve, we may mention those which Sir John Herschel made during his stay of four years at the Cape of Good Hope, at Feldhausen, where he examined and measured 2100 double stars of the southern hemisphere, of which a few only were before known.

A brilliant series of observations is due to the English astronomer Dawes, who, from 1831 to 1867, made no less than 2762 observations on double stars.

We may finally give a description, in chronological order, of Otto Struve's observations, son of the celebrated William Struve in Russia; of Mädler, in Prussia and in Germany; of Admiral Smyth and Lord Wriottesley, in England; of Captain Jacob, in the Southern hemisphere; of Kayser, at Leyden; of P. Secchi, at Rome; of Powell, in the Southern hemisphere; of Fletcher, in England; of Dembowski, at Gallarate, near Milan; of Brunnow, at Dublin; of Knott and Wilson, in England; of Winlock, at Harvard College (United States); and of Burnham, at Chicago—who, by their researches, their observations, their micrometrical measurements, and their discoveries, have successively endowed astronomy with accumulated riches.

Such are the principal astronomers who have specially devoted themselves to the *observation* of double stars. Others have been occupied with calculations of the orbits of binary systems. The French astronomer Savary was the first who, in 1830, applied a method of calculation to these determinations. Encke afterwards proposed a method much more complicated than that of Savary, which appeared to him to lead to more accurate results. Sir John Herschel, on the

contrary, endeavoured afterwards to simplify the calculations, and published, in the "Memoirs of the Astronomical Society of London," a method almost entirely graphic, still leaving, however, much complication in the final formulæ. Successively the astronomers Bessel, Mädler, Hind, Smith, Villarceau, De Gasparis, Wolf of Zurich, and Klinkerfues proposed analytical methods, all of which necessitate the longest and most exact calculations. A little disheartened by these complications, and being convinced that they were not necessary to obtain accurate results (the measurements of double stars not themselves admitting of this precision), I tried, in 1874, to solve these problems more simply, and to find graphically the absolute orbit from the apparent orbit by the assistance of simple trigonometrical formulæ. I have succeeded, and this method is not only incomparably more simple, but even more sure, than the preceding, although it does not pretend to the same degree of precision.*

The historical series of these labours shows how science has progressed in less than a century to the profound knowledge of the partial stellar systems, and above all of binary systems.

III. *Nature of Double Stars.*

When a star shows itself double in the field of a telescope, it is not certain on that account that it is a true double star of which the components are associated. The junction observed may be merely an effect of perspective, and one of the two stars may be in reality very far behind the other in space, although found on the same visual ray. In

* My method does not give the period in thousandths of a year, but the principal number is exact. Thus, for example, the preceding methods have given, with pretension to the thousandth of a year (which I abstain from reproducing), the following periods for σ *Coronæ* :—

737	years	Hind.
608	,, ...	Maedler.
420	,, ...	Klinkerfues.
240	,, ...	Powell.
195	,, ...	Jacob.
843	,, ...	Darerck.

Evidently of these six periods five at least are imaginary. These calculations —in which the method of least squares plays the principal part—act blindly, and give mechanically divers results, according to the figures which are manipulated. Thus no greater precision must be attached to micrometric results than they deserve. For γ *Virginis* we have results which vary from 629 years to 139. My method gives 175 years, to nearly half a year, and it cannot be wrong. But it cannot be applied to couples for which the measurements are insufficient.

this case there is only an optical group, and the two stars are not physically associated.

To obtain the data necessary to determine the nature of a double star, it must be observed with care during a great number of years, and the relative position of the two components measured as exactly as possible, to ascertain their state and their position. When we possess these precise observations made at intervals of ten, twenty, or fifty years, and concordant intermediate observations, we find either that the two components have remained fixed, stationary, invariable in position one to the other, or else that their relative position has changed, and that they are respectively displaced. In this last case it is necessary to examine carefully the form of the movement. We take the most brilliant of the two components as a point of comparison—we suppose it to be immovable, and we place the second star at the different points observed during the series of years of observation. If these successive positions follow a curve round the principal star, this curve is studied, the missing part is obtained by calculation, and we find that the little star turns round the large one, following an ellipse more or less elongated. This is the apparent orbit as seen from the earth. Often instead of forming a curve, the displacement observed forms a right line. In studying this rectilinear movement, it is found at times that it also resembles an ellipse very much elongated, of which the plane passes through our visual ray, and instead of turning, the secondary star appears to oscillate in a right line from one side to the other of its primary. A wheel which is seen turning in front shows us a circular movement; seen obliquely, it offers an elliptical movement; seen all together from the side, it shows a rectilinear movement, and if it were transparent we should see a mark made on its circumference simply ascend and descend above and below the mean position. The examination of the rectilinear movement shows sometimes, on the contrary, that it is owing to a proper movement of one of the two stars in space, and that the second far distant star rests immovably in the depths of the heavens. There are even many cases where a star passes before two or three others much farther off and immovable. Sometimes these stars pass so exactly one before the other that they eclipse themselves. When they are of different colours, this eclipse of one star by another, so unexpected and so new to science, offers new phases to which no other celestial phenomenon can be compared. There are thus in this

world of double stars an immense variety of movements, to which may still be added the effects produced by the sidereal translation of the star system across the changing perspective of the starlit space. It is thus found that there are amongst the double stars couples possessing definite orbital movements, optical couples, due only to the chance of perspective and to our arbitrary position in space, and couples which have remained fixed since their discovery. Do these last represent true systems of stars associated by a physical bond, or are they only perspective groups? Both species can be found among them. When the components of a double star—the whole having preserved the same relative position with regard to each other—are brought together into space by a mutual proper movement, it is thereby evident that they are mutually associated. But do they necessarily turn one round the other, in an extremely slow movement, which may not reach 5° in a century (for this variation would be noticed), and the duration of which may exceed an average of seven thousand years, and may extend, perhaps, to ten, twenty, fifty, or a hundred thousand years? This is probable, at least in the great majority of cases. But although the laws of universal gravitation appear to oppose all other hypotheses, and although astronomers have until lately declared that it must be so, we think that there are cases in which two or more stars might be brought near together by a proper movement in space, without thereby turning round a common centre of gravity situated within their group. This extraordinary fact is proved by the analysis that I have made (among others) of all the observations and of all the measurements taken of 61 *Cygni*. This beautiful double star is one of the most celebrated in the entire heavens, because it is the first of which the terrestrial homunculus had the audacity and the glory of determining the distance. Its parallax being 0·51″, its distance is 403,600 times the distance from here to the sun, that is to say, 14,933,200 millions of leagues. At the rate of 77,000 leagues a second, light would have to fly for six years and five months to cross the abyss which separates us from it. Well! this star is double, and it has been observed as such since 1753, that is to say for 123 years. The illustrious Bessel believed he was able to calculate its orbit, and had estimated its period at 400 years. Since then this period has been raised to 450, 520, and even to 600 years, and its mass has been supposed to be calculated. By a singular chance, it was precisely on the supposed orbit of this famous star that astronomers at

first based their arguments on the universality of gravitation; and, moreover, to-day we find it described in all treatises of astronomy as being the most interesting of the orbital systems. But on reuniting the hundreds of micrometrical measurements taken during this long interval of time, I have arrived at the conclusion that the two components of 61 *Cygni* do not turn round each other at all, but that they are carried together in space by a rapid proper movement, which, whilst it is common to both, removes them slowly from each other. These two suns are truly associated, but do not gravitate round each other, and their relative displacement is performed in a right line. This proper movement is one of the most rapid that we know; it is 515″ in a century, a velocity which represents a minimum of *many millions of leagues a day.* These two suns should be one behind the other in perspective, and advance or retire from us in such a way that their line of junction would appear more in face, and their angular distance appear to augment. This system is not the only one of its kind; I have found many others like it. Thus there are stars associated by a proper common movement which do not gravitate round each other.

Amongst the couples of which the components remain not only fixed with regard to each other, but altogether immovable the one to the other at the same point of the celestial sphere, many can form physical couples, and many optical couples. Their distance in the depths of infinity, the slowness of their absolute or relative movement, will only permit us to recognise their nature after centuries of continued observation. Absolute rest does not exist in the immense universe.

Many very distinct causes thus operate on the double stars to give this real or apparent movement: the rotation of the components of a binary, tertiary, or multiple system, round their common centre of gravity; the gravitation of two or more stars carried together in space under the influence of unknown sidereal attractions; the differing proper movements of two stars much separated one behind the other in the line of the visual ray; the secular translation of our solar system in space, which reacts by giving to less distant stars an apparent displacement in a contrary direction.

In giving a general review of the double stars, I have found 614 systems having certain orbital movement, and for each of these I have traced the figure of the observed movement. Thirty couples have travelled over a part of their orbit

sufficiently considerable to admit of their orbit being calculated and the period examined. We have even been able not only to measure the light, but also to find the mass of many of these stellar couples, and we thus know that our sun is but one of the smallest stars, and that a certain number of those which shine so modestly under the discreet veil of the silent night are in reality more bulky, more heavy, more resplendent and hotter than this gigantic focus on whose rays the life of our small planet hangs. These groups of suns in orbital movement are connected mutually by the bonds of a reciprocal attraction, and execute their revolutions in closed curves following the laws discovered by Kepler and explained by Newton. Before observation, in our century, had discovered their existence, we only knew similar movements in our solar system, in the translation of the planets round the sun, the satellites round the planets, the periodical comets round the same focus of attraction. We now know that the force which governs our family, which extends with decreasing power as far as the last planet of our system, as far as Neptune, and even twenty-eight times farther, since the solar attraction still governs at 131,000 millions of kilometres the great comet of 1680, retains it in its orbit, and forces it to return, . . . we know, say I, that this force reigns also in other universes, that it is truly universal, and that it governs in the depths of infinity stellar systems the farthest from us.

Stars which, until our day, were called fixed, are, on the contrary, those which move the most rapidly, and the flight of a cannon ball is as the slowness of a tortoise compared to them. The knowledge of these partial systems, whose movements are accomplished without any outward influence, opens to one's thoughts a field so much larger that already these systems appear in their turn like mere details in the vast *ensemble* of movements which animate celestial space.

When one of two associated suns possesses a mass much more powerful than the other, it appears to be the centre of the movement, as our sun appears to be the centre of the movement of translation of the earth and the planets, although in reality the planets and the sun itself turn together round their common centre of gravity, a mathematical point the position of which varies constantly, and which is usually situated in the interior of the sun. The smaller of the two stars revolves round the greater, and no spectacle is more imposing than these sidereal revolutions. In some systems the revolution is made in less than half a century; for

example, the couple of stars of the northern crown are two golden suns, of which the cycle is forty years. In some systems the period approaches a century, as in that of 70 *Ophiuchi,* which is composed of a clear yellow sun and a rose sun, which gravitate round each other in a cycle of 92 years. The brilliant couple of γ *Virginis* is composed of two equal suns, which revolve slowly round themselves, and which together turn round their common centre of gravity in a period of 175 years. The tertiary system of ζ of Cancer is composed of three suns: the second turns round the first in a cycle of fifty-eight years, and the third round the two others in six hundred years, describing epicycloidal curves which I discovered at the beginning of the year 1874, and which much perplexed me, as also the astronomers to whom I communicated them at the time. We know, finally, orbital systems, such as those of γ *Leonis,* ϵ *Lyræ,* and of the polar star, of which the cycle surpasses a thousand—and even many thousands—of years. Others move more slowly still. Thus the double stars are so many stellar dials suspended in the heavens, marking, without rest in their majestic silence, the inexorable march of time, which passes there as here, and showing to the earth, from the depths of their unfathomable distances, the years and the centuries of other universes! Eternal clocks in space! Your movement does not stop; your finger, like that of Destiny, points out for other beings the ever-turning wheel, now rising to the height of life, and now plunging into the abyss of death! And we, in our lower sojourn, must read on your perpetual movement the sentence of our terrestrial lot, which will carry along our generation like a grain of dust flying over the paths of heaven, whilst you continue to turn in silence in the mysterious depths of infinity!

IV. *The Number of the Double Stars.*

Double stars are not an exception in Nature; their number, as compared with that of simple stars, is, on the contrary, well worthy of attention; and I only speak here of true multiple stars, not of merely perspective groups. To arrive at an approximate knowledge of this comparative number, I at first examined separately all the stars of the first, second, and third magnitude, and then arranged them in the order of their decreasing brightness, and in each case I noted *its sidereal nature.* The result has been that, of the 300 most brilliant stars of heaven, there are—

		Proportion.
Simple stars	222	0·74
Really multiple stars . . .	78	0·26
	300	1·00

The numerical proportion between the multiple stars and the simple stars is 26 per cent—that is to say, one-fourth of the 300 selected stars.

Is the proportion the same for those of inferior magnitude? To ascertain this I have examined separately 2615 multiple stars described in the great catalogue of Dorpat, and 770 from the catalogue of Pulkowa: they extend from the first to the ninth magnitude. I have found that below the eighth magnitude the progression diminishes, and that to obtain comparable results we ought to stop at the seventh. Deducting those optically double, we find that 2340 physical couples have been noticed in the celestial sphere from the north pole to 150° of southern declination, from the first to the seventh magnitude. In the same extent of the celestial sphere there are the following number of stars:—

1st magnitude	. . . .	11
2nd ,,	. . . .	34
3rd ,,	. . . .	106
4th ,,	. . . .	325
5th ,,	. . . .	1000
6th ,,	. . . .	3800
7th ,,	. . . .	12500
	Total	17776

The proportion of these numbers should give us about two double stars to every fifteen stars. It is certain that this proportion is too low, because the most brilliant stars have been observed more frequently and examined with more care than the less brilliant ones (although at Dorpat and Pulkowa they have scrutinised the heavens with the express aim of finding double stars. Without the perseverance which was devoted to scrutinising so attentively the neighbourhood of Sirius and Procyon, we should not have discovered their satellites. The diminution of the multiple stars below the fourth magnitude may also be due to the increasing feebleness of the satellites, in proportion as the primary ones are themselves smaller or farther off. If we consider the stars of the entire heavens, in order of brightness, we arrive at the following proportion:—

	Number of Stars.	Simple.	Multiple.	Proportion. S.	Proportion. M.
1st magnitude	18	13	5	0·72	0·28
2nd "	59	44	15	0·75	0·25
3rd "	180	136	44	0·76	0·24
4th "	550	435	119	0·79	0·21
	807	628	179	0·76	0·24

The proportion of the multiple systems to the simple stars diminishes gradually with the magnitude, and very probably, as we have just explained, partly on account of the diminution of brightness of the satellites, and partly owing to the less number of observations. We ought, then, to consider that we are very near the truth in taking the average of the first three magnitudes for the general average—that is to say, 74 simple stars and 26 multiple stars in every hundred. Hence we arrive at the following general conclusion :—

The suns, which constitute the fundamental bases of the universe, *are either simple suns*—like that which illuminates us—*or systems composed of two or more suns* associated together by a physical bond. The number of simple suns is greater than that of multiple suns. The proportion between the two appears to be, in round numbers, 3 to 1. One-fourth of the stars of heaven is formed of multiple stars.

If the universe contains 75 millions of stars from the first magnitude to the smallest telescopic magnitude, it is then by *millions* and by tens of millions that we must count the multiple stars. Far from being an exception in nature, they, on the contrary, play a considerable part in the general organisation of the universe.

V. *Multiple and Coloured Suns.*

Our white and solitary sun—our solar system formed of a single focus, round which gravitate obedient worlds following regular orbits—does not constitute the type and model of universal creation. There are multiple suns shining with various hues, sometimes varying their colours, sometimes opposing one another, sometimes successively alternating them in the same sky ; suns of dissimilar volume and mass, acting sometimes in the same direction, sometimes in two contrary directions, to drag out of shape the singular orbits of the unknown worlds which gravitate under their power. No sight is more magnificent than the telescopic contemplation of these strange suns. When during the silent night,

during the immense repose of Nature, in these hours of night when the humanity surrounding us is sleeping in anticipated death, our looks and our thoughts rise by means of the marvellous telescope towards these celestial lights which shine on high for other worlds, and radiate around them heat, activity, and life, the contrast is so great that we seem to be in a dream. Here is death, above is light; here is lethargy, above is movement; here is shadow, above is splendour; here is heavy and dull matter, above is devouring flame and sidereal life. How poor is our sun by the side of its grand brothers, of its elders in space! How miserable is our world by the side of those which float above on the rapid and multiplied wings of such an attraction! What delicious hours might contemplative minds and curious souls pass directing a telescope towards the heavens, if the most learned men, if the cleverest women in the world, were not universally ignorant of the most elementary facts of astronomy, and if they did not always live and revolve in the same monotonous circle, without troubling themselves about the marvels that divine Nature holds in reserve for those who comprehend them!

Direct, for example, a telescope towards the beautiful star of the second magnitude, γ Andromedæ. It appears to the naked eye like an ordinary star; but suddenly, to telescopic vision, it shines on the dark background as a double sun, orange and green. Nothing is more striking than this effect. The contrast is splendid, and nothing more beautiful can be imagined. To myself the double star γ Andromedæ, Saturn's ring, and the mountains of the moon in her first quarter, are the three most profound and the most captivating recollections which remain to me of my first observations in astronomy. With a sufficiently powerful telescope we distinguish, at the side of the little emerald-green sun γ Andromedæ, a third—smaller, and of a sapphire-blue colour. We have, then, under our eyes a remarkably curious and wonderful triple star: for a hundred years that the two principal stars here coupled have been observed, they have remained immovable and in the same place with regard to each other, at 63° N. and at 11″ angular distance. The third star was only discovered in 1842, and since that time it has already revolved 26 degrees. If this movement continues on an average, it will revolve round the emerald sun in a period of about 460 years. The orbital movement of the green and blue couple round the orange sun would be incomparably more slow, according to the third law of Kepler (the squares of the time are to each other as the calculated

distances—AB = 11″, BC = 0·7), and would only be accomplished in a cycle of many thousands of years. It is possible, then, that the fixedness of the two orange and green suns may be only due to the insufficiency of time for observations; a variation of 3 degrees would be sensible on this couple. The period might extend to twelve thousand years.

One of the most magnificent of the coloured double stars is the star Albires, or β Cygni, topaz and sapphire, not less admirable than the preceding. To the naked eye this is merely a simple star, of the third magnitude. In the telescope it is a celestial couple of great beauty. The large sun shines with a strong and deep yellow colour; the little star appears modestly at its side, of a pensive blue. The contemplation of this ethereal marriage brings to mind the Paradise of Milton, where the poet sings of male and female stars commingling their regards and light across the heavens:—

> . . . . "Other suns, perhaps,
> With their attendant moons thou wilt descry,
> Communicating male and female light."

In the chromatic scale these colours are exactly complementary; moreover, when examined separately, they are real, and not due to an effect of optical contrast. Examined with the spectroscope, the two components of the couple give two different spectra; that of the golden yellow sun is crossed by a series of black absorption lines, occupying its blue part; that of its companion presents an analogous system, occupying its yellow and red region. These stars owe their colour to the nature of the vapours in their atmospheres. Like γ Andromedæ, the couple β Cygni is fixed. It has been observed since 1755, and find the little blue star at 66° 34″ from the topaz star.

Another double star, very beautiful as to its colours, is ε Bouvier, which is called also Pulcherrima, a star of the third magnitude seen with the naked eye, and composed of an orange star and a small brilliant emerald star. The two components are only 3 seconds apart, and it requires a good instrument to separate them. They form an orbital system having a slow movement.

The star α Hercules, also of the third magnitude, may be equally quoted as being one of the most beautiful in the heavens (orange and green). It is stationary at 119° 4″. The brilliant star Antares, of the first magnitude, is red, and this colour can be observed with the naked eye. On a clear and transparent night the attentive eye is sometimes struck by some green rays which furtively mix with its

scintillation. In the luminous neighbourhood of this brilliant star has been discovered a little companion completely green, and it is not impossible that sometimes these rays, traversing those of Antares, mingle more or less together. Castor is composed of a yellow star and a second slightly greenish, the reflection of which sometimes modifies the hue of Castor seen by the naked eye.

We may also notice, as types of beautiful coloured stars, the groups of o Eridan (orange and blue), 32 Eridan (yellow, topaz, and greenish blue), 2 of the Triangle (yellow and green), β Scorpionis (yellow and blue), η Persii (red and blue), ξ 163 (orange and sapphire), 35 Pisces (pale yellow and violet), η Cassiopeæ (pale yellow and lilac), δ Orionis (pale yellow and blue), ϕ in Taurus (red and blue), θ Herculis yellow, green, and purple), α Scorpionis (copper, red, and clear blue), ϵ Pegasi (golden yellow and azure), γ Dauphinis (cadmium-yellow and variable pearl gray), 95 Herculis (pale green and variable cerise-red), &c. All the colours of the rainbow are represented in the polychromatic illuminations of these far-off suns. Sometimes there are not only two stars which arrest the astonished gaze by the marvellous contrast of their colours, but three, four, five, six, or more; the most admirable example of these masses of multi-coloured stars is situated near the star χ of the southern cross; we can there count not less than 110 stars collected together at the same spot of the heavens, sparkling in all colours, like a casket of precious stones.

The contrast of colours is one of the most remarkable characteristics of the double stars. In 600 couples chosen from Struve's great catalogue, and carefully observed, we find 375 in which the two stars present the same colour and the same degree of intensity; 101 in which the two stars are also of the same colour, but with a difference of intensity: and 120—that is to say, a fifth of the total number—where the colours are completely different. Of these last have been counted 52 where one of the two stars is red and the other very green or very blue; 52 where the first is pale yellow and the second pale blue; and 16 where one is green and the other blue.

In examining the differences of magnitudes of the components in proportion to their differences of colour, we find the following proportions, which are very significant:—The 375 stars of which the colour is the same, with the same intensity, give, for their average difference of magnitude, the number—

0·416.

The 101 stars of which the components are of the same colour, but of different intensities, give for the same difference—

1·144.

And the 120 stars of which the components are of different colours give the number—

1·883.

The disproportion is such that there can remain no doubt that difference of colours is so much stronger in proportion as the difference of the magnitudes is itself greater. The greatest intervals of magnitude are not between the yellow and blue, but between green and blue stars.

Now are all these colours real, or will there not rather be, in most cases, an effect of contrast? A white star placed by the side of a very brilliant red star becomes green by contrast. The retina of the eye, excited by light of one determined colour, becomes insensible to a less intense light of the same colour, and only sees the complementary colour.

Red produces green.
Orange ,, blue.
Yellow ,, violet.
And reciprocally.

To know if these colours are real, and not due to the effect of contrast, it is necessary, when possible, to hide one of the stars while looking at the other; the effect of contrast disappears with its cause, but not immediately, as is generally believed. It takes a certain time—many seconds, and sometimes nearly half a minute—for the eye not to be influenced. It is for that reason difficult to experiment when the two stars of a couple are at a small angular distance. The diurnal movement magnified by the eye-piece quickly causes the star to emerge from behind the wire which has been placed before, even when the telescope has an equatorial movement. But the experiment is easy if we take stars which do not touch. One of the easiest of the examples, even for a moderate instrument, is that of the triple star o^2 Cygni. The large star is of the fourth magnitude; the second, of the 6·5th magnitude, is distant 1′ 47″; and the third, of the fifth magnitude, is at a distance of 5′ 38″. The diversity of the colours is surprising; the first is of a fine yellow, and the other two are blue. If, then, we hide the largest, and look only at the two little ones, they entirely retain their blue colour. The same observation

may be made on o^2 Eridan (orange and sky-blue), separated 82″; on β Cygni, of which we have just spoken in reference to the magnificence of their colours, and of which the two components are 34″ apart; an α of the *Hounds* (pale yellow and pale blue), the separation of which is 20″; &c.

Experiment is evidently of use only when the colours are complementary and belong to the two opposite ends of the spectrum—that is to say, where one is yellow, orange, or red, and the other is green, blue, or violet. But if the two are red, or one red and the other yellow,—or if the two are blue, or one green and the other blue,—there is no interfering effect of contrast, and these colours are incontestable.

It is unanimously affirmed (Arago, Humboldt, J. Herschel, &c.) that there is not a single green or blue star by itself in the entire heavens, and that this colour is only met with in the double stars. Having discovered (in my minute and detailed analysis) optical groups in which one of the stars is blue, I have discussed them with the greatest attention, to be sure that these cannot be physical groups. My labours have resulted in the inevitable conclusion that simple blue stars do exist.

Let us quote, for example, the beautiful couple of the double star ζ 2908 Cygni (7th, golden yellow; 8·5th, blue) of complementary but real colours. Their distance apart has augmented from 9″ to 21″ since 1834, and the little blue star has displaced itself in a direction contrary to the proper movement recognised in the large one. In reality the little one is relatively immovable in the sky, and the larger one nearer to us is passing before it. Their distance was at the minimum in 1795. This is only a perspective group, and the two stars are not related. There are, then, simple stars, which are blue.

Among the stars visible to the naked eye I do not know of one which is green or blue. There are white, yellow, and red stars, but none which are of a green colour. β Lyræ has sometimes shown a pale tint; this is the only case which exists, and again it is not authenticated; for whilst many eyes consider it yellow, others see it white, and others somewhat green. The exact estimation of colours is more difficult than is imagined, especially in the case of stars, for their colours are in general faint and transparent. Among the brilliant stars Antares, α Herculis, Mira Ceti, β Pegasi, δ Persii, Pollux, α Orionis, and Aldebaran are red: Arcturus, Castor, the Pole star, Capella, Procyon, β in the Little Bear, α in the Great Bear, are yellow; Sirius, Vega, α Cygni, the cluster in Virgo, Rigulus, Rigel, ε, ζ, η in the

Great Bear, are white. This whiteness is so bright that for certain stars they appear even slightly tinted with blue,

To compare the colour of the stars with each other, and to deduce results applicable to the colouration of the double stars, I have invented an apparatus which forms optical couples with all the simple stars of the firmament, by bringing them as near together as may be desired, and which measures the difference of their tints by the aid of appropriate glasses. This apparatus, to which I have given the name of the Chromascope, and which is founded on the principle of the sextant, has allowed me to compare, one with the other, most of the stars visible above the horizon of Paris, including the planets, employing now and then artificial light to facilitate the comparison. The results which I have obtained are very curious. Thus, for example, a gas-burner seen at 1 kilometre off produces—with Sirius in the telescope—a garnet and sapphire couple; Mars and l'Epi, a red and azure couple; Mars and Saturn, a red and green couple; Saturn and Altair, a yellow and blue couple; the Moon and Sirius, a brass- and silver-coloured couple; Antares and Vega, a red and blue couple; &c.

These comparisons have enabled me to classify the stars in a decreasing order of colour, beginning with red. Here are some examples:—

1. μ of Cephi	Red-orange.
2. Lighting gas	Orange-red.
3. Antares	Orange.
4. Mars	,,
5. Mira Ceti	,,
6. α Herculis	Orange-yellow.
7. Pollux	Yellow-orange.
8. Aldebaran	,,
9. Bételgeuse	,,
10. Polar star	Full yellow.
11. Capella	Yellow.
12. Castor	,,
13. Jupiter	,,
14. Arcturus	,,
15. Procyon	,,
16. The Moon	Creamy yellow.
17. Rigel	Yellowish white.
18. Mercury	White.
19. Sirius	,,
20. L'Epi	,,

21.	Vega	Bluish-white.
22.	Altair	,,
23.	ξ in the Great Bear . . .	,,
24.	Saturn	Yellowish-green.

What astonished me most in these researches was to find Mars less red than a gas-flame. I have renewed the experiment more than thirty times before being convinced of this.

By combining the first stars with the last I have formed couples which reproduce, nearly in all their intensity, the double stars red and green, yellow and blue, described above, such as γ Andromedæ, β Cygni, &c. Must we therefore say that it is necessary to suppress all the green, blue, or violet colours of the double stars, and to attribute these colours only to the effect of optical contrast? Undoubtedly no; but we must be sure that these stars are extremely rare, that many of them have their intensity increased by contrast, and that a very large number only owe their hue to the effect of contrast.

In these comparisons it is not only the tint of the stars which produces these contrasts, but the different intensities of the lights play a part which is far from insignificant.

In summing up, therefore, although contrast increases the difference of the colours of the multiple stars, these colours really exist, and all the tints of the spectrum are represented in the illumination of the suns of the universe. But the colours at the red extremity of the spectrum are much more frequent than those at the blue extremity. From the point of view of the effects produced on the unknown worlds which gravitate round these many-coloured suns, we may remark, moreover, that the effects of contrast may augment the real colours, when two or more of these suns appear together above the same horizon. This is what I purpose contemplating in the following chapter.

VI. *Worlds Governed and Illuminated by many Suns.*

Ought we to suppose with a learned contemporary astronomer—M. Faye, Member of the Academy of Sciences, and President of the Bureau des Longitudes—that the idea of the plurality of worlds is a mediocre idea ("Year-Book" for 1874, p. 477); that in all our planetery systems there is only Mars which is habitable; and again (*Id.*, p. 487), that the double and multiple suns are incapable of governing inhabited planets (*Id.*, p. 484); and that for a world to be

habitable it is necessary that it should be in just the same astronomical, meteorological, geological, and even geographical condition (*Id.*, p. 485) as the earth which we inhabit? Ought we not, on the contrary, to feel that it would be presumption on our part to impose limits to the power of Nature, and, above all, to take our little planet as the absolute type of creation?—presumption to pretend that the entire universe must be only a vast and useless desert, and that, because we are here collected round our grain of dust, it follows of course that the whole creation is here accumulated? Can we suppose that prolific and inexhaustible Nature, who, long before the appearance of man, peopled our poor planet with beings in climatalogical conditions quite different from those which to-day exist,—who filled the ocean with living beings, contrary to all the deductions of Science previous to the discovery of these beings,—who formed vegetable and animal life with exhaustless profusion on the face of the entire globe, from the polar snows to the heat of the tropics,—and who shows us the earth as all too narrow a field for the life which overflows it all around; ought we to suppose, say I, that this same Nature is everywhere else barren and sterile, and that all the suns of infinity are shining, uselessly spreading round them a light which illuminates *nothing*, a heat which warms *nothing*, an energy which brings forth *nothing?*—attraction, light, heat, magnetism, physical and chemical forces, scattered without object and without effect over the immensity of the universe! When we know that the earth is only an atom in creation, can we pretend that it alone bears on its surface beings who feel, eyes which see, thoughts which elevate them towards Truth, and that the rest of infinity must be void, dumb, and blind? No, a thousand times, *No*. Never shall we come to such a conclusion from the study of Nature; never shall we be led to suppose that our miserable grain of dust constitutes in itself alone all infinity, and contains in this imperfect fragment the whole power of the eternal Creator.

What! Do all these splendid suns which we have been studying exist without an object? Do all these sidereal revolutions which have given them birth, these conflagrations of atoms, these fertilisations of worlds, these appalling geneses, these gigantic displays of power, these systems ruled by immutable laws,—all these powers, all these splendours, have they . . . . only brought forth a mouse? Still less—since the earth does not belong to their system—must we say that all this exists in vain? Must we decree

all this to be by us *useless?* Shall we declare all this superfluous, and perhaps even wearisome? We! the atoms of an atom; *we*, blind gropers in the dark; *we*, ignorant dwellers in a cave, who will only believe in the existence of the pale mushrooms which surround us! By what name can designate such pretension? Moreover, this is possibly the reasoning which is indulged in on other planets, where they declare that the earth must be uninhabitable because it is too cold (supposing it to be the inhabitants of Mercury who talk), or because it is too hot (supposing it be the Jovian inhabitants who hold forth). Poor Earth! a microcosm invisible to Jupiter; lost, like a little spot on the sun, to Saturn and the other exterior planets; unknown to all the other solar systems of infinity;—who would believe that it is still declared the queen of creation? The queen, did I say? Who would believe that, as at the time of Joshua, we even now persuade ourselves that we are *alone* in the universe, and that nothing exists beyond our narrow boundaries?

This is not our mode of reasoning. On the contrary, without doubting for a moment that the stars, the suns of space, may be, in general, the centre of planetary systems more or less different from ours, we shall here discuss the novel conditions of worlds governed and illuminated by many suns, of different masses, different volumes, and different colours.

What is the character of the orbits described by the worlds belonging to these singular systems? Do these unknown planets turn round the two suns as their common centre? and have they, as the focus of their movements, the centre of gravity of the twin suns? or, rather, has each of the two suns its proper planetary system? This last should be most probable and most general.

Notwithstanding the essential difference which exists between these systems and ours, we can, however, make use of the disposition of our system to divine the possible arrangement of the others. Already in our system one planet surpasses all the others in its volume, and without doubt also in its intrinsic heat, and it forms the centre of a little system of four moons, which it carries with it during its eleven years' revolution round the sun. Let us suppose that Jupiter, which is already 1400 times larger than the Earth, was of a still more considerable volume, and shone with a blue light; this supposition alone would modify our planetary system to the extent of creating three species of worlds:—First, four globes (the satellites of Jupiter), one of which is larger than the planet Mercury, illuminated and

governed by a primary blue sun, and receiving at the same time the more distant illumination of our actual sun; secondly, three immense worlds, Saturn, Uranus, and Neptune, turning round a double sun, one white and the other blue; thirdly, four moderately-sized globes, Mars, the Earth, Venus, and Mercury, turning round the white sun, but illuminated at certain times during the night by a second blue sun. Let us now endow the sun with a red light; we thus reproduce one of the most common types amongst double stars coloured with complementary shades. Let us examine for an instant what a succession of phenomena would be produced by the modification which we have thus made in the sun and in Jupiter.

Let us imagine the Earth as it would be at the time of Jupiter's opposition, that is to say, placed just between the sun and Jupiter. In this position, there would be no night for any point on the Earth; whilst the red sun shone on one side of the Earth, the blue sun would shine on the other; there would thus be a red day on one hemisphere and a blue day at the other, and all the meridians of the globe would pass successively in twenty-four hours through these two kinds of day, distributing to all the countries of the globe twelve hours of red day and twelve hours of blue day, with no night.

But our blue sun would not itself remain stationary in space; it would slowly turn round the red sun; soon it would rise before the other had set, and would appear above the eastern horizon when the celestial ruby had not yet disappeared. The blue day would then succeed, but the sapphire sun not setting in its turn before the rising of its shining rival one, we should have a night for a few moments ornamented with two auroræ boreales of a new kind, the one reddish at the east, the other bluish at the west. The duration of this night would augment from day to day, and at the same time as the double day illuminated by the two suns at a time, the blue hours and the red hours diminishing in the same proportion. Ultimately, and at the epoch corresponding to the conjunction of Jupiter, the blue sun would approach the red sun, and there would be neither red nor blue day, but a double day followed by complete night.

The light of the double day would be formed naturally by the union of the colours of the two suns. It would be violet, but might be quite white, if the colours were complementary. Carried on always by its proper movement, the secondary sun would pass to the west of the first, and would soon produce blue mornings, followed by a white day, or a

mixture of a red evening, and a night becoming shorter and shorter, until the blue sun came back to opposition, as it was situated at the commencement of this description.

In most of these systems of double stars, the little star turns round the larger, not in a circle, but describing an elongated ellipse. The stability of the system demands that this little star should not approach too near to the larger one, because, in this case, supposing, as is most likely, that the planets circulate in the same plane as the stars themselves, these planets might be attracted by the central sun at the moment of the perihelion passage, and abandon their primitive sun, to the great detriment of their inhabitants, who would doubtless be burnt up before the astronomers of these worlds had been able to definitely prove the desertion. It is indispensable that these systems be very well restrained round each of the two suns, and that the obedient gravitating planets be kept close under the protecting wing of their own special sun. But in all cases there must be on them the most singular alternation of seasons.

Thus in all planetary systems governed by a double sun, our double alternation of day and night is replaced by a quadruple alternation; 1, a double day lighted by two suns at a time; 2, a single day lighted by a single sun; 3, another single day lighted by the other sun; 4, finally some hours of complete night, when the two suns are together below the horizon.

And the splendour of these natural illuminations can hardly be conceived by our terrestial imagination. The tints which we here admire in these stars can only give a distant idea of the real value of their colours. Already in passing from our misty latitudes to the clear regions of the tropics, the colours of the stars deepen, and the heavens become a veritable casket of precious stones. What would this be if we were transported beyond the limits of our atmosphere? Seen from the moon, these colours should appear splendid. Antares, α Herculis, Pollux, Aldebaran, Bételgeuse, Mars, shining like rubies; the pole star, Capella, Castor, Arcturus, Procyon, would be celestial topazes, whilst Sirius, Vega, and Altair would shine like diamonds, eclipsing all by their dazzling whiteness. What, above all, would this be if we could get near enough to them to distinguish their luminous discs, instead of seeing only brilliant points divested of all diameter.

Blue days, violet days, red days, green days! For us to form an idea of these strange effects, let us mentally change for an instant the colour of our sun, and suppose that

instead of the white source of all light which bathes us, a blue light spread over the earth. What a change would instantly come across the face of nature! The clouds losing the silver and golden hues, and spreading under the heavens in a sombre vault; the face of Nature veiled in a coloured penumbra; the most beautiful stars remaining visible by day in the heavens; flowers hiding the brilliancy of their lustrous adornment; the fields bathed in gloom to the invisible horizon; a new day dawns in the heavens; faces become old, and the human race, with astonishment, demands an explanation of so strange a transformation. We know so little, beneath the surface, of things, we go so much by appearances, that the entire universe would seem renewed by this slight modification of solar light.

How would it be if, instead of a single indigo sun following with regularity its apparent course, bringing the years and the days by its single government, a second sun were suddenly to unite itself to it; a blood-red sun disputing always with its partner the empire of the world of colours? Imagine that at midday, when our sun spread over nature the penumbral light we have just described, the fire of a resplendent focus lighted the east with its flames. Greenish shadows pass suddenly across the diffused light; and opposite to every object a dark train obliterates the blue mantle spread over the world. Later, the red sun rises as the other sets, and objects are coloured to the east with red rays, to the west with blue. Later still, a new day dawns upon the Earth, while as the first sun sets, nature becomes bathed in a brilliant fiery red. As night approaches, hardly does west begin to fade like far off Bengal fires in the last rays of the purple sun, when a new morning dawns at the opposite side, tinted with the colours of the Cyclopean blue eye. Can the imagination of poets or the caprice of painters create on the pallet of fancy a world of light more daring than this. Hegel has said, that "what is real can be imagined," and that "what can be imagined is real." This daring thought does not quite express the whole truth. There are many things which do not appear rational to us, and which, nevertheless, exist in reality in the other numberless creations of the infinities which surround us.

There are, no doubt, living eyes there which contemplated these marvels each day. Who knows? It is very probable that they scarcely pay any attention to them, and being from their cradles accustomed, as we are, to the same things, they do not appreciate the picturesque beauty

of their lives. In such a manner are we also constituted, the new and the unexpected only affect us. What is natural appears to us to be an eternal necessity, an accident of blind nature, which is not worth the trouble of being observed. If one of those habitants of the far-off world could come to us, whilst they would recognise the simplicity of our little universe, they could not fail to observe with surprise and astonishment our indifference to it.

If like our moon, which gravitates round the globe, and those of Jupiter and Saturn, which together shine on the darkened hemisphere of these worlds, the invisible planets which yonder balance themselves are surrounded by satellites, which accompany them, what ought to be the aspect of these moons lighted by many suns? This moon, which rises behind yonder mountains, has quarters of different colours, at one time red, at another blue; now it is a yellow crescent, and now at full, it is green, and appears suspended in the heavens like an immense fruit. Ruby moons, emerald moons, opal moons! What singular lustre? O nights of Earth, so modestly silvered by our solitary moon, ye are very beautiful when contemplated by the calm and pensive mind! But what is your beauty compared to nights illuminated with those marvellous moons?

And what sort of eclipses of the sun would there be on these worlds? Multiple suns, multiple moons, to what an infinite play of colours would not your mutually eclipsed lights give birth! The blue sun and the yellow sun approach each other, their joint lustre produces green on surfaces illuminated by both, and yellow or blue on those which only receive one light. Soon the yellow sun draws near the blue, it eats into its disc, and the green, spread on the pale world, fades and fades till it dies, melting in the golden hue which pours its crystalline rays in space. A total eclipse colours the world yellow! An annular eclipse shows a blue ring round a gold medal. By degrees, insensibly, the green shines out, and, recovering its empire . . . . . Let us add to this phenomenon that which will be produced if a moon, coming in the middle of this gilded eclipse, covers the yellow sun himself and plunges the world in obscurity; then, following the relation existing between its movement and that of the sun, it continues to hide it after the emergence of blue disc, and then leaves nature to repose again under the obscurity of an azure veil! Let us still further suppose . . . . . but no, we have encroached upon the boundless treasury of nature, remaining inexhaustible in spite of all our efforts.

VII. *Stellar Systems, of which the Orbits are Calculated.*

Amongst the thousands of multiple stars studied, there are only a very small number of which the observations are, on the one hand, sufficiently numerous, and the movements sufficiently rapid, in the next place, for it to be possible to determine the elements of their orbits. In order that our readers may understand the nature and the varieties of these orbits, we collect together here the systems which have, to the present time, been able to be calculated, writing them in order, increasing with their periods. As a rule, each of these orbits has been calculated by many astronomers, and the results have shown considerable divergence. The elements which we publish here are the last calculated. The periods which are not absolutely sure are marked thus (±)—.

DOUBLE STARS WHOSE ORBITS HAVE BEEN CALCULATED.

Names.	Magnitude.	Half-Major Axis.	Periods. Years.	Colours.	Calculators.
42 ξ Capella	6—6	0·66″	25·71	White	O. Struve, 1875
ζ Hercules	3—6	1·19	34·57	Reddish yellow	Flammarion, '73
η Northern Crown ..	5·5—6	0·86	40·17	Yellow	Flammarion, '74
ζ in Cancer (tertiary system) AB	5·5—7	0·91	58·80	White	Flammarion, '74
ζ in Cancer (tertiary system) AC	5·5—6	5·00	700 ±	White & yell.	Flammarion, '74
ξ Great Bear	4—5	2·45	60·60	Golden yellow & ashy	Flammarion, '73
α in Centaur	1—2	15·50	77·00	White	Powell, 1870
70 p. Ophiuchi	4·9—6	4·78	92·77	Yellow and rose	Flammarion, '74
ξ in Scorpion	5—5·3	1·20	98·00	White	Flammarion, '75
γ Austral Corona.. ..	6—6	2·55	100·00	White	Capt. Jacob, '58
ω of the Lion	9—7	1·05	136·00	Yellow	Klinkerfues, 1870
3062 ζ Cassiopeia.. ..	7—8	1·00	147 ±	Yellow and olive	Maelder, 1850
ξ of Bouvier..	3—6	9·95	169 ±	Yellow and orange	Hind, 1872
γ Virgo	3—3	3·38	175·00	Golden yellow	Flammarion, '74
η Cassiopeia	4—7·5	10·68	176·00	Yellow and lilac	Duner, 1875
δ Cygni	3—8	1·80	178·00	Yellow & violet, variable	Hind, 1864
τ Ophiuchi	5—6	1·11	185·00	White	Doberck, 1875
36 Ophiuchi	4—6	4·00	200 ±	Red and yellow	Admiral Smyth, 1860
1757 ζ Virgo	8—9	2·00	240 ±	White and yellow	Admiral Smyth, 1860

Names.	Magnitude.	Half-Major Axis.	Periods. Years.	Colours.	Calculators.
λ Ophiuchi	4—5·5	1·20	250 ±	White and ashy	Capt. Jacob, '58
μ^2 in Bouvier	8—8·5	1·50	290·00	Green	Doberck, 1875
36 Andromedæ	6—7	1·54	349·00	Orange and yellow	Doberck, 1875
γ Lion	2—4	2·00	402·00	Yellow	Doberck, 1875
ε of θ Hydra..	3—4	3·00	490 ±	Yellow and purple	Admiral Smyth. 1860
μ in the Dragon	4—4·5	3·00	600 ±	White	Hind, 1865
49 in the Serpent.. ..	6—6·5	3·00	600 ±	White and yellow	Klinkerfues, 1870
12^2 in Lynx	6—6·5	1·50	700 ±	White and red	Maelder, 1850
γ Coronæ	6—6·5	6·00	843·00	White & ashy	Doberck, 1875
Castor	2·3—3	7·74	996·00	Yellow	Thiele, 1872
ε^1 Lyræ	5—6·5	3·00	1000 ±	Yellow and orange	Maelder, 1850
ξ in Versian..	4—4·5	7·64	1578·00	White	Doberck, 1875
ε^2 Lyræ	5—5·5	2·90	2000 ±	White	Maelder, 1850.

We see in this table that there is a great variety of periods, and although those which are inscribed here extend over 2000 years, there are much longer periods which are still only vaguely estimated. We may remark that the colours of the stars in rapid orbital movement do not offer the beautiful contrasts of which we were speaking before. This is a surprising fact, and one which has greatly astonished me; *the most beautifully-coloured stars turn only very slowly round each other.* There is not a single contrast in the thirty-one systems of this table; α Cygni only seems to offer an exception; but its colours are undecided and variable. One can remark, also, that amongst these movements some are direct, that is to say accomplishing in the direction east, south, west, north, and whilst others go backward (west, south, east, and north), one of the orbits, that of ξ Scorpionis, is very inclined on our visual ray; this makes the orbit appear very elongated. Another, that of 42 Capella, lies entirely in the plain of the visual ray, so that we only see a waving movement of it. As can be easily verified, the greatest variety reigns in these stellar systems.

VIII. *Masses of Double Stars—How we Weigh a Star.*

The double stars are not only immense suns shining with their proper light in the depths of the heavens, but we know now that they are heavy, bulky, ponderous, and incom-

parably heavier than the earth on which we are, heavier even than our colossal sun.

The power which we have of weighing a star is, unquestionably, one of the most surprising results of the progress of science, one of those which leaves the greatest doubt in the mind of persons unaccustomed to the principles of celestial mechanism. To weigh a star is a work even more extraordinary than to measure its distance, and neither Copernicus, Galileo, Kepler, nor Newton certainly ever dreamt the day was coming when their successors would be capable, by the application of their immortal discoveries, to determine the mass of a star lost in the depths of celestial space. Therefore, we think we ought to complete this summary statement of our actual knowledge of the double stars by indicating the results arrived at on the magnitude and masses of these systems, and in giving an idea of the method employed to obtain them.

The mass of a star is calculated by the energy of the action which it exercises around itself. If the earth was ten times heavier than it is, preserving always the same volume, it would attract bodies towards its surface ten times more strongly than it does now, and an object which in falling travels over 16 feet in the first second of falling would travel 160 feet. If the Earth, whilst preserving its volume, had the mass of the sun, it would attract bodies 324,000 times stronger; and an object which now weighs 1 kilog. would weigh 324,000. A man of the average weight of 70 kilog., would weigh 22 millions! We can measure the weight of a star by the intensity of the attraction at its surface. Reduced to its simplest expression in its application to the fall of bodies, this attraction would be difficult to verify; but we can determine it by the speed of a satellite revolving round the star of which we wish to know the mass.

For example, the attraction of the earth has the power of curving the right line, which would be followed by the moon in space if it did not follow this attraction; and it curves by attracting it in such a manner that it makes a circumference travelled over in 27 days 7 hours 43 minutes. If the mass or energy of the earth were to augment, the velocity of the moon in its orbit would increase also: if it were to diminish, the contrary effect would be produced. The attraction varies in direct proportion with the masses. The quickness of the moon's movement round the earth comes from the actual force of the earth. The earth is the hand which makes the moon turn in opposition. If the earth had more power, more energy than it has, it would make the

moon turn quicker, and *vice versa.* The same occurs in respect to the sun and the earth. If the sun were increased in weight, the earth and the other planets would turn more quickly round him, and the years would diminish in length. If the mass of the sun were diminished, the contrary would happen. It is by comparing the action of the sun on the earth with the action of the earth on the moon, that we have found that the sun is 324,000 times more energetic, more powerful, more heavy than the earth.

If, then, we had in space a celestial couple, the mutual distance of the two components of which was equal to that which separates the earth from the sun, or 92 millions of miles, the examination of the duration of its revolution would immediately give us the mass of the system relatively to that of the sun. It is easy to show that if a couple of celestial bodies turning round their common centre of gravity employs a certain time, T, to accomplish its revolution, whilst another couple, of which the components are at the same distance from each other, employs another duration of time, D, to accomplish its revolution, the mass of the first is to the mass of the second in the inverse proportion to the square of the time, that is to say as D^2 is to T^2.

If the distance is not the same, it is necessary at first to reduce it to this equality, by bearing in mind the law which regulates the distances. "The squares of the times are to each other as the cubes of the distances."

I have been able to calculate the mass of the stellar system of the double star 70 p. Ophiuchi. By the combination of all the observations, I have found that the period is 92·77 years, or 92 years 283 days. The parallax of this star being 0·168″ corresponds to a distance from the earth equal to 1,400,000 times that of the sun. At this immense distance, the size of the terrestrial orbit being reduced to the preceding angle, the half major axis of 4·88″ represents 1075 millions of leagues. This is a little less than the distance from Neptune to the sun. A planet situated at this distance from the sun will accomplish its revolution in 156·55 years. The proportion between them is then as $(156{\cdot}55)^2$ is to $(92{\cdot}77)^2$, or as 2·89 is to 1, whence I have concluded that *the mass of the Ophiuchus system is nearly three times superior to that of the sun* and Neptune unitedly, or (Neptune having only a small mass) to that of the sun alone. Thus there is a star, scarcely visible to the naked eye, which weighs 900,000 times more than the Earth.

Let us remark, in passing, that the orbital movement of the small star round the large one is 191,830 leagues a day,

or 8881 metres a second, and that these two twin suns travel through immensity with a speed of which the minimum is 246 millions of leagues a year! And these are the stars which were but yesterday called *fixed stars!*

Calculations made on the other stars have led to equivalent results, showing us these celestial torches as gigantic and heavy suns, the enormous distance which separates us from them reducing them to simple mathematical points. The star nearest to us, α Centauri, has a parallel of 0·91″, and, in consequence, its distance from the earth is about 8 trillions 376 millions of leagues. If we adopt 15·5″ for the average value of the angle comprised between the two components, we find that their distance apart is 2520 millions of trilometres, or 630 millions of leagues; that is, less than the distance between Uranus and the sun. Its period appears to be about 77 years. The result is that it weighs a little less than our sun, and that in representing its mass by ten, that of the sun should be represented by twelve. But its volume ought to be greater (we always speak of the two stars united), because its intrinsic light is about three times superior to that of our sun. If we were to consider the quantity of light as a criterion of the surface of emission, we should find that the diameter surpasses that of the sun in the proportion of seventeen to ten.

The period of Cassiopeia, which at first was estimated at 700 years, then at 181 years, has now been fixed at 176, and it is probable that this number is not very far from the truth. In admitting the parallax to be 0·154″, which removes it from us 55 trillions of leagues, and the semi-major axis to be 10·68″, the mass of this system will be ten times that of the sun.

As to the mass of 61 Cygni, which we find described in all astronomical works, it is wrong, and, as we have already seen, it is impossible to determine, considering that the two components do not turn round each other.

Thus the double stars are truly suns, gigantic and powerful, governing in the regions of space illuminated by their splendour systems of worlds different from those of which we form a part. The sky is no longer a gloomy desert; its antique solitudes have given place to regions peopled like that in which the earth gravitates; the obscurity, the silence, the death which reigned in these heights has given place to light, to movement, and to life; the thousands and millions of suns pour lavishly into space energy, heat, and the diverse undulations which emanate from them as a focus; all these movements succeed each other, cross each

other, fight against each other, or unite in the incessant maintenance and the development of universal life; the universe is transfigured to our minds; suns succeed to suns, worlds to worlds, universes to universes; formidable movements carry all these systems through the boundless regions of immensity; and everywhere to the farthest boundary where fatigued imagination can rest its wings, everywhere developing in its infinite variety, we meet with divine creation, of which our microscopical planet is only an imperceptible province.

V. ON THE COLOURING OF THE SHELLS OF BIRDS' EGGS.

By H. C. Sorby, F.R.S., &c., Pres. R.M.S.

ONE of the interesting results due to the spectrum method of research, as applied to various biological subjects, is that there is far greater unity and simplicity in the elementary colouring matters than might have been expected from the very various tints of the natural objects. Their number is indeed very considerable, and in some cases, though the colour may be nearly the same, the substances may be most materially different. Very often, however, we meet with an almost endless variety of tints, due not to any essential difference in the elementary colouring matters, but to the varying proportions of a few well-marked substances. This is very well illustrated by the colour of many eggs—even, perhaps, of the same species of bird—which may, for example, pass from greenish through all shades of brown to reddish, brought about by the varying development of a blue and of a red constituent; or may, in other eggs, pass from blue to very yellow green, owing to a variation in the relative amounts of a blue and of a yellow colouring matter.

Another interesting fact connected with this subject is that there is often a close and remarkable relation between different classes of objects and their characteristic coloured constituents. This is strikingly the case with the more important of those met with in the shells of birds' eggs, since, so far, I have never met with them in any other natural products. Further research may, indeed, lead to their discovery elsewhere; but, at all events, birds' feathers of very similar colour contain entirely different substances. Much remains to be done before the subject can be looked upon

as fully understood; but, still, I have examined sufficiently numerous examples to be able to form a tolerably good general opinion. The detail would require much further research. A paper on the subject, published in the "Proceedings of the Zoological Society of London," May 4, 1875, p. 351, gave a full account of what I had been able to learn up to that date, and since then my attention has been directed to entirely different subjects, so that I am unable to add much to what I then said, and can merely speak with greater confidence respecting some closely allied subjects. On the whole, I cannot do better than make some extracts from this paper, with any further illustrations that may appear desirable.

Hitherto I have been able to distinguish eight well-marked substances. One of these is identical with a colouring-matter met with in nearly all groups of plants, from the lowest to the highest; another is probably the black pigment found in feathers; but I have not yet been able to identify any of the rest with any found elsewhere. But, at the same time, I must admit that our knowledge of animal colouring-matters is far too limited to make such negative evidence of much value. All these seven coloured substances found in the shells of birds' eggs are insoluble in water, but soluble in absolute alcohol, either when neutral or when a small amount of free acid is present. They are also sometimes soluble in chloroform or carbon bisulphide. Neutral or acid absolute alcohol, however, is in every respect the most convenient and best solvent. Some are extremely permanent, and resist the action of powerful reagents, whereas others are of such unstable character that they are not only rapidly changed by acids or oxidising reagents, but are even partially decomposed by evaporating their solutions to a dryness at a gentle heat. In these general peculiarities they resemble bile-pigments more than any other group of colouring-matters, but do not actually agree with any that have passed under my notice. Some of them furnish us with a number of most interesting facts in illustration of the probable existence of a connection between optical properties and chemical or molecular constitution; but on the present occasion I forbear to enter into such questions, and will confine myself as much as possible to the zoological aspect of the subject. At the same time, it is absolutely necessary to enter into a certain amount of chemical and optical details, since otherwise the characteristic peculiarities of the different substances could not be established.

Some important and reliable information may be learned

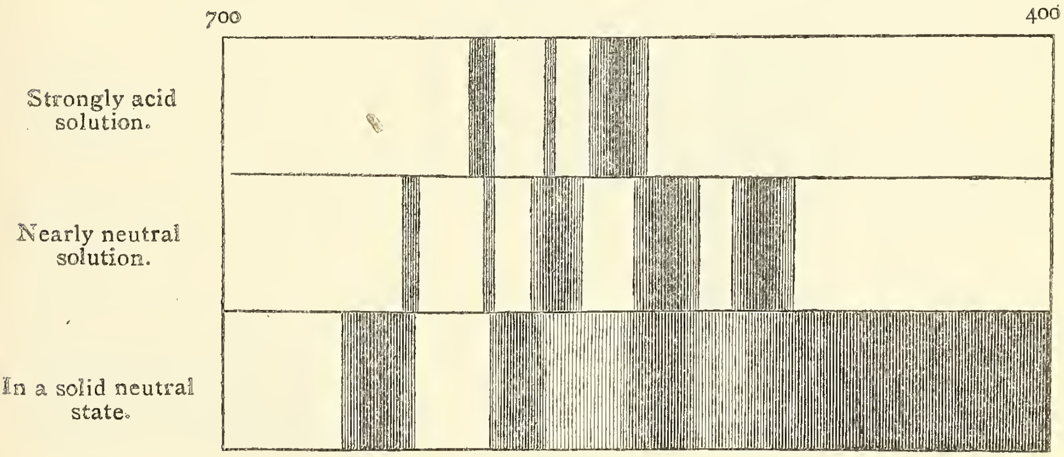
from the spectrum of the light reflected from the eggs themselves or transmitted through broken fragments; but in order to study colouring-matters in a satisfactory manner, it is requisite to obtain them in solution, so that they may be more or less separated from one another, their spectra seen to greater advantage, and the effect of various reagents determined. In the shells of eggs the coloured substances are so intimately associated with carbonate of lime that they cannot be dissolved out; and even when this has been removed, they are often so firmly enclosed in other insoluble organic substances, that it is difficult or impossible to dissolve them out completely. In the majority of cases it is best to remove the earthy carbonates by means of somewhat dilute hydrochloric acid, added gradually until no further effervescence takes place. The character of the residue varies much in different cases. Sometimes we obtain a coloured membrane, occasionally like dark morocco leather, whilst in other cases the membranous part is very pale, and the colour chiefly occurs in detached skin-like flocks, or as minute particles disseminated through the liquid; collecting this insoluble portion on a filter, washing well with water, and removing any large portions of colourless membrane and of the filter to which no colouring matter is attached, the coloured residue should be freed from superfluous water, but not actually dried, and placed whilst moist in absolute alcohol. This usually dissolves out a considerable amount of the colour; but some still remains insoluble. A portion of this is occasionally soluble in alcohol containing free acetic acid; but very often much remains undissolved until the residue is treated with alcohol containing hydrochloric acid. Sometimes even this fails to remove all, even when heated for many hours. All these different solutions should be kept separate, since they usually differ most materially; and in no case should a strong acid solvent be used unless found to be necessary, because several of the normal colouring-matters are rapidly decomposed by strong free acids. For this reason it is in some cases advisable to separate the carbonate of lime from the shell by means of acetic acid; but then unfortunately the colouring-matters are much less readily dissolved out of the residue by alcohol.

These general remarks will, I trust, suffice to indicate the character of the methods usually employed; and I therefore now proceed to describe the different coloured substances hitherto met with. The number of species of eggs which I have been able to carefully study is less than I could have wished; but what I have already been able to learn suffices

to explain the more obvious peculiarities of the colours of eggs, since those examined were carefully selected for this purpose.

1. *Oorhodeine.*—This is perhaps the most important and interesting of all the colouring-matters, not only because it gives a number of most interesting spectra, of such a well-marked character that a very minute quantity can be recognised without any difficulty, even when mixed with a relatively large quantity of coloured impurities, but because it occurs, in large or small amount, in the shells of such a great number of eggs that its entire absence is exceptional. When in a perfectly neutral condition it is almost insoluble in alcohol; so that when the washed shell-residue is digested in cold absolute alcohol very little is dissolved, until a small quantity of hydrochloric acid is added. On evaporating this solution to dryness at a gentle heat, and treating it at once

FIG. 1.



Spectra of Oorhodeine.

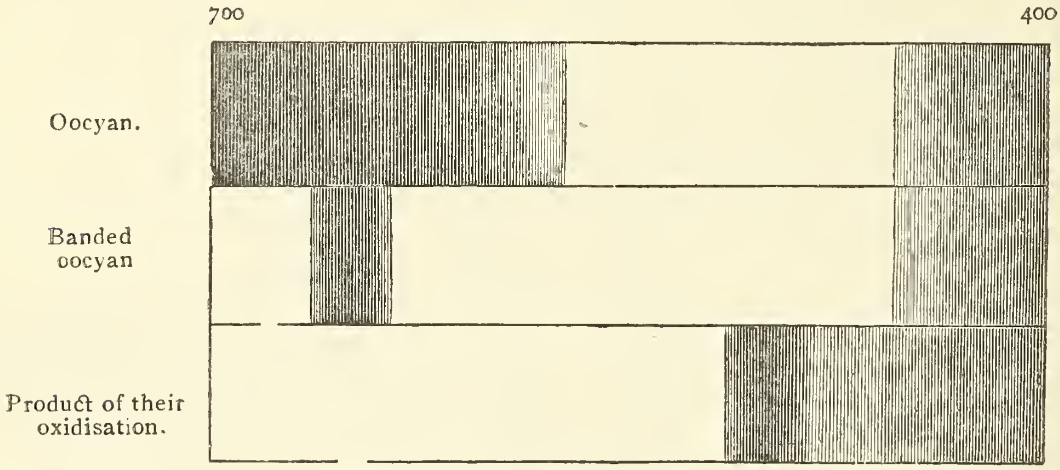
with absolute alcohol, a considerable part dissolves, probably because a small quantity of acid clings to it; but if a small excess of ammonia be added, and the solution again evaporated to dryness, the neutral residue is all but insoluble in alcohol. These peculiarities enable us to separate oorhodeine from most of the other colouring-matters, and to obtain it approximately pure. It gives spectra with extremely well-marked absorption-bands, which differ in number, character, and position according to the conditions in which it occurs. The more important of these spectra are shown by fig. 1, in which, as well as in all the other figures, they are given, not as seen with a *prism*, but as they would appear in an *interference* spectroscope, since in that case alone do we see the true relations of the different parts.* To any one

* We are indebted to the kindness of the Zoological Society for the use of the woodcuts illustrating this paper.

accustomed only to an ordinary spectroscope the blue end will therefore appear abnormally contracted and the red end expanded unusually. The numbers given at the top represent millionths of a millimetre of wave-lengths of the light.

As will be seen, the strongly acid solution gives a spectrum with three bands, two of which are so well marked that a most minute quantity of the substance serves to show them in a satisfactory manner. When as small a quantity of free acid is present as will enable the alcohol to dissolve oorhodeine, the spectrum shows the five absorption-bands given in fig. 1, and the general colour is brownish-red. This in the spectrum of the almost neutral modification when in a state of *solution*. When in a free *solid* form, as in the shell, or as found in the washed dry skin-like residue after removing the carbonate of lime by an acid, the spectrum is most materially different, as will be seen from the woodcut. Only three

Fig. 2.



Spectra of the Oocyans, &c.

bands are distinctly visible, and they lie nearer to the red end, whilst there is far more of general absorption at the blue end. The result of this is that the general colour is a peculiar brown-red.

Oorhodeine is of such a very permanent character that it resists the action of very powerful reagents. I have been able to destroy it, but have not yet succeeded in changing it into any other coloured substance.

2. *Oocyan.*—In most cases this is readily soluble in neutral alcohol, and can thus be separated from oorhodeine. It is, however, often associated with yellow substances that cannot easily be removed; very commonly, therefore, the solution is of a somewhat green-blue colour, but in many cases the yellow impurity is far more easily decomposed by the action of light or by weak oxidising reagents, and can be removed by this means, so as to enable us to determine the true

colour and spectrum of the oocyan itself. When dissolved in alcohol it is of a very fine blue colour. The spectrum shows no detached bands, but a strong general absorption of the entire red end and of a small portion of the extreme blue, as shown in fig. 2.

3. *Banded Oocyan.*—This also is of fine blue colour, but differs from the former species in giving a spectrum with a well-marked detached absorption-band near the red end, as shown in fig. 2. It is also far less soluble in neutral alcohol, so that it is left in the shell-residue after having been digested for some time in cold neutral alcohol, and can subsequently be dissolved out by alcohol, to which a minute quantity of hydrochloric acid has been added. The solution must, however, be examined at once, since banded oocyan is rapidly decomposed by strong acids.

Both these different kinds of blue colouring-matter are evidently in a state of very unstable equilibrium. Sometimes the greater part of the colour is lost by merely evaporating their solutions to dryness at a gentle heat; and several very interesting products can easily be obtained by acting on them with reagents.

On adding a moderate excess of hydrochloric acid to a solution of oocyan, no other immediate change occurs than the destruction of some of the yellow substances that may be present; but in the case of *banded* oocyan, two new faint bands are developed in the orange and yellow end of the green, and it is gradually changed into a new modification, or perhaps even into a new substance, characterised by giving a spectrum with two bands, quite unlike that of the original. On adding to the solution of banded oocyan a little hydrochloric acid and potassic nitrite, it is rapidly decomposed into an orange-coloured substance, giving a spectrum with a simple well-marked absorption-band between the green and blue, as shown in the figure. In the case of oocyan this same substance is also produced: but there is an intermediate red compound formed, characterised by giving a spectrum with two bands (one in the orange, and the other at the yellow end of the green), which, however, do not correspond to those of the product of the action of acid on the banded oocyan.

It will thus be seen that these two blue colouring-matters (oocyan and banded oocyan) differ in very important particulars, but are obviously closely related, since they both yield the same well-marked product when oxidised.

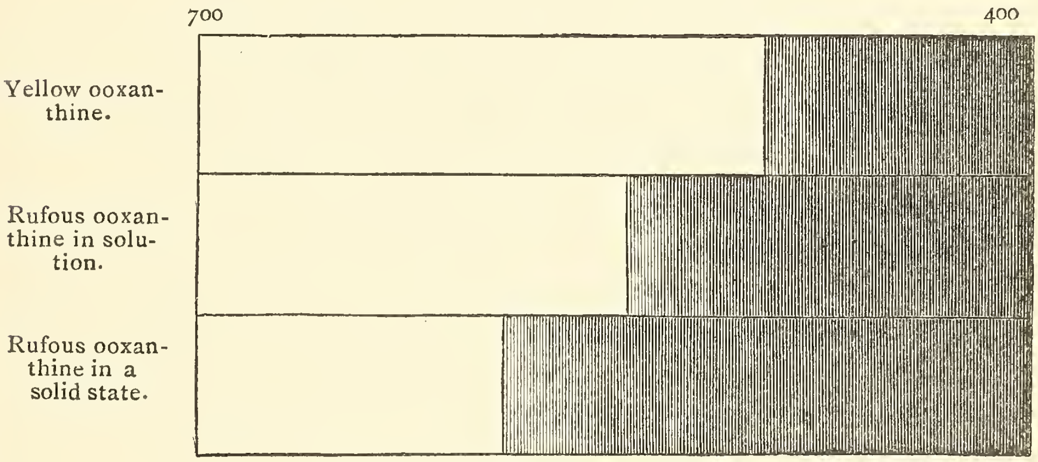
4. *Yellow Ooxanthine.*—This substance may be best obtained from moderately fresh Emu-eggs. These are of a

fine malachite green colour, due to a mixture of yellow ooxanthine with oocyan. On completely dissolving out the carbonate of lime with moderately strong hydrochloric acid, the residue is of deep green-blue colour, and a large part of the ooxanthine is decomposed by the action of the acid. On the contrary, if the carbonate of lime be dissolved out by acetic acid, nearly all the oocyan is lost, and a yellow residue is obtained, coloured by yellow ooxanthine, which, however, is so firmly associated with the thick tough membrane, that it is almost impossible to dissolve it out in alcohol. If, however, the shell be partially dissolved in dilute hydrochloric acid, a yellow layer is formed on the surface, which may be detached from the greener part below, not yet free from the earthy matter; and this yellow layer easily gives up part of its colour to neutral alcohol, and a further quantity to alcohol containing a little acetic acid. These solutions are of a clear yellow colour, giving a spectrum with no detached bands, absorbing the whole of the blue light, and strongly transmitting nearly all the green and the whole of the red end of the spectrum; that is to say, light of less wave-length than 500 millionths of a millimetre is absorbed, and of greater wave-length transmitted. In a solid state, in the egg-shell, the absorption extends down to wave-length 508. Alkalies and weak acids produce no immediate change in the solution; but a strong acid like hydrochloric rapidly decomposes yellow ooxanthine, and leaves only a pale, almost colourless residue of another substance, which will be described in the sequel. This change takes place immediately if a minute portion of potassic nitrite be added to the acid solution. The alcoholic neutral or acetic solution is also rapidly decolorised by exposure to direct sunlight. Hence it will be seen that this yellow substance is in a state of very unstable equilibrium, and is rapidly decomposed by oxidisation, when a strong acid is present in a free state, or when exposed to bright light.

5. *Rufous Ooxanthine.*—Hitherto I have not met with this substance in any other eggs but those of the different species of Tinamou, and have studied it more especially in those of *Rhynchotis rufescens*, in which it occurs associated with much oocyan. It agrees with yellow ooxanthine in being rapidly decomposed by a strong free acid, and immediately when a little potassic nitrite is added: but it is not so easily, if indeed at all, destroyed by the action of a moderately dilute aqueous solution of hydrochloric acid; and its presence does not seem to have any effect in decomposing the oocyan; whereas yellow ooxanthine has a most remarkable influence,

since, as will be apparent from what I have already said, when the carbonate of lime is dissolved out by a weak acid the whole of the oocyan disappears if the amount of yellow ooxanthine is considerable, whereas no such decomposition occurs when it is absent. Rufous ooxanthine also differs from yellow ooxanthine in absorbing light to a very considerably greater distance from the blue end. Even when dissolved in alcohol it absorbs not only all the blue, but also at least one-half of the green; that is to say, all light of less wave-length than 550 millionths of a millimetre is absorbed, and all of greater wave-length transmitted, which, of course, is a very well-marked difference, as will be seen on comparing the spectra given in fig. 3.

FIG. 3.



Spectra of the Ooxanthines.

When in a solid state in the egg the absorption extends considerably further towards the red end, down to wave-length 590 or thereabouts; so that the tint is decidedly red, and not the orange-colour of the solution or the bright yellow of yellow ooxanthine. When mixed with oocyan, it therefore gives rise to the peculiar lead-colour of the eggs of the rufous Tinamou—and not to green, like that of the fresh eggs of the Emu.

6. *Substance giving narrow absorption-bands in the red.*—Unfortunately I have not yet succeeded in obtaining this in sufficient quantity, or sufficiently free from other substances, to be able to decide whether its true colour is blue, green, or brown; but the fact of its giving a spectrum with several narrow absorption-bands in the red would certainly indicate that, when mixed with other colouring-matters, it would cause them to have an abnormally browner tint. Small quantities of it occur in very many eggs; but I have not yet found it so abundant in any as to exercise a more important

effect on the general colour than to make it somewhat more dull. Since the entire spectrum is not accurately known, I will merely give the position of the different very narrow absorption-bands in millionths of millimetres of wave-length. The most complete spectrum shows three bands. On adding excess of ammonia, that nearest the red end alone remains, whilst the addition of a small excess of a strong acid removes all but the central band—and when the excess is considerable, raises this band towards the blue end. These facts will be better shown by the following table :—

	Centre of bands.		
Most complete spectrum . . .	668	648	628
With excess of ammonia. . .	668	—	—
With a little acid	—	643	—
With much acid	—	641	—

By means of these bands a very small quantity of this substance can easily be recognised. It is not readily decomposed—but, when acted upon with oxidising reagents, may be changed into another colouring-matter, giving rise to a spectrum with one or two somewhat obscure bands.

7. *Lichnoxanthine.*—In my paper on comparative vegetal chromatology, I have described a substance which occurs in greater or less amount in almost all classes of plants, from the lowest to the highest, but is more especially abundant in and characteristic of lichens and fungi, and for this reason has been named by me *lichnoxanthine.* The spectrum shows strong general absorption of the blue end down to about wave-length 510 millionths of a millimetre, and a much weaker general absorption down to about 590 millionths. Acids and alkalies produce very little change ; and it is very slowly altered by strong oxidising reagents. I have been able to prepare it artificially by the decomposition of resins. Some such substance is undoubtedly present in small quantity in very many kinds of birds' eggs ; and occasionally there is so much as to materially modify the general colour. It may occasionally have been, to some extent, derived from the decayed vegetable matter of the nest, or, in the case of eggs which have been kept long, may be partly due to the growth of minute fungi ; but, at the same time, a very closely allied, if not identical, substance does really appear to be a normal constituent of the shell of eggs having a peculiar brick-red colour. Very probably it may not be formed in the animal organism, but absorbed directly from the vegetable food of the birds. This need not in any way surprise us, since it is a substance of great stability, and there is strong

reason to suspect that many yellow feathers are coloured by the far less stable xanthophylls derived from that source. The loose yellow pigment on the beak of the hornbill, with which he paints his feathers and makes himself look a far smarter bird than he otherwise would be, corresponds in every particular with a mixture of xanthophyll with lichnoxanthine and a little yellow xanthophyll, all of which could be so readily absorbed from the vegetable food along with fatty or waxy substances, that it seems unnecessary to suppose that they are formed in the bird's own organism, though this is of course quite possible.

Such, then, is a general account of those peculiarities of the colouring matters that have come under my notice, which suffice to distinguish them from one another and from analogous substances met with elsewhere; and I now proceed to a more detailed consideration of the eggs themselves. As an illustration of the method of study, suppose that we have taken portions of the brownish-red eggs of the common Grouse, of the pure brown eggs of the Nightingale, and of the pure blue of the common Thrush, separated from the black spots, kept for examination by themselves. After having, in each case, dissolved out the carbonate of lime with dilute hydrochloric acid and having washed the residues with water, they should each be digested in cold neutral absolute alcohol. Scarcely any colour would be dissolved out in the case of the Grouse—but a fine blue in all the others, which, on further examination, would be found to be oocyan, with mere traces of other substances. After having dissolved out as much as possible, by means of fresh neutral alcohol, the residue should be digested in alcohol with a small quantity of hydrochloric acid. It would then be found that the Grouse-shell would give a rose-coloured solution, containing much of the acid modification of oorhodeine. The Nightingale would also give much oorhodeine, but the colour would be modified by the presence of oocyan; the blue portion of the Thrush-egg would give a small quantity of a fine blue substance, showing the spectrum of banded oocyan, with little or no trace of oorhodeine, whereas the dark spots would be found to give a very considerable quantity of oorhodeine. We thus clearly see that the redder egg is mainly coloured with oorhodeine; the blue egg with oocyan—the brown colour of the Nightingale being due to a mixture of these two, and the black spots on the Thrush-egg to patches containing much oorhodeine. All the various intermediate shades of colour, passing from red through brown to blue, whether they occur in the eggs of different

species or in the more or less variable eggs of the same kind of bird, or in patches on the same egg, can thus be explained without any difficulty.

In a similar manner the various shades of green, passing from the blue-green of such eggs as those of the common Hedge Sparrow to the fine malachite green of the fresh Emu, and to the very yellow-green seen on them in patches, are all due to a variable mixture of oocyan with yellow ooxanthine.

As is no doubt well known, many green eggs turn blue on long keeping. In this manner the beautiful malachite green of fresh Emu-eggs passes into dark blue. This is easily explained by the fact that yellow ooxanthine is much more easily destroyed by oxidisation than oocyan. A portion of a green Emu egg exposed to strong light soon becomes much bluer, and so does a mixed solution of the two colouring-matters in alcohol, the yellow constituent being destroyed and the blue left.

A few eggs are of a brick-red colour. Those of Cetti's Warbler are as good an example as any I have seen ; and on carefully comparing them with the browner red egg of the common Grouse, I found that both contained a large amount of oorhodeine, but that the tint was made more dull in the case of the Grouse by the presence of a small quantity of the colouring-matter which gives the narrow bands in the red ; whereas in the case of Cetti's Warbler this was almost or quite absent, and there was present a relatively very unusually large amount of the orange-coloured substance, which I have not been able to distinguish from lichnoxanthine. To the presence of this substance we may thus attribute the brick-red tints seen in a few eggs.

The eggs of the black variety of the common Duck are coloured with a nearly black substance, which I have not yet obtained in a state of solution, and probably corresponds to the so-called *pigmentum nigrum*, which may be obtained in larger quantity in a more satisfactory condition from black hairs or feathers, and used for drawings as a splendid black pigment, quite equal to the best Indian ink.

My studies of colouring-matters by the spectrum method soon led me to perceive that the individual species of certain groups of coloured substances are so intimately connected with their life that plants may be arranged in a kind of natural order according to the presence, absence, or relative proportion of the various coloured constituents, which order on the whole agrees remarkably with that founded on structural characters, as shown in my paper on comparative

vegetal chromatology.* There is also reason to suspect a connection between the evolution of one colouring-matter from another, or from colourless constituents, and the progress of the general organisation. These facts naturally led me to consider whether any such connection could be recognised in the case of birds' eggs. Much remains to be learned before any positive opinion can be expressed; but what is already known appears to be sufficient to prove that, if there be any definite connection between the general organisation of birds and the coloured substances found in their eggs, it is not of such a kind as is at all obvious to any one who, like myself, is not thoroughly acquainted with anatomical details. Six out of the eight different colouring-matters occur in variable amount in a very great variety of eggs, but there is no greater variation than is met with in the different individual eggs of the common Guillemot; so that the study of the colouring-matters cannot be looked upon as of any value in distinguishing species, or even much wider groups, except, perhaps, in one particular instance. Hitherto I have met with rufous ooxanthine only in the eggs of the Tinamous, and perhaps in those of some species of Cassowary; and though the question needs further examination, it is desirable to give a short account of what is already known.

As previously described, rufous ooxanthine when in solid form in the shell of such redder Tinamou-eggs as those of *Crypturus obsoletus* (Wickham), absorbs the blue, the green, and some of the yellow rays, but transmits the orange and red; so that the colour is a sort of orange-red, made duller and of more leaden tint in the eggs of other species by mixture with oocyan. The result is that we obtain tints which are not so decidedly different from those due to a mixture of oocyan with oorhodeine as to lead any one to conclude at once that they were not due to the same substances. However, when the eggs in their natural state are properly illuminated by light so condensed on them sideways from a lamp that as little as possible is reflected from the surface, the spectra are seen to differ entirely. When oorhodeine is present, one or more of its absorption-bands may be seen; but when the red colour is due to rufous ooxanthine, no trace of any such bands can be recognised. My knowledge of the chemical and optical characters of rufous ooxanthine when in a state of solution were derived from the study of the eggs of the rufous Tinamou (*Rhynchotis rufescens*); and hitherto I have been able to study only the spectra of the

*Proceedings of the Royal Society, vol. xxi., p. 442.

eggs of other species through the kindness of Mr. Osbert Salvin, on whose authority I give the various names. Taking all the facts of the case into consideration, it appears to me to be almost certain that the redder-coloured constituent in all the different species is rufous ooxanthine. At all events, none show any trace of the bands of oorhodeine, and all show the same absorption of light of less wave-length than about 590 millionths of a millimetre. All that remains to be done to make this point certain is to examine the *solutions* derived from other species than that I have named, in order to be sure that the chemical as well as the optical characters are identical. In the present state of the question the following conclusions must be looked upon as only extremely probable.

No species of Tinamou yet examined contains any recognisable amount of oorhodeine. The colour of many species is due to a variable mixture of rufous ooxanthine with oocyan, the former greatly preponderating in such red eggs as those of *Crypturus obsoletus*, *C. pileatus*, and *Nothoprocta curvirostris*. The red and blue constituents occur in more equal proportion in the peculiar lead-coloured eggs of *Rhynchotis rufescens*. *Calodromas elegans*, when in a comparatively fresh state, contains so much yellow ooxanthine that it is pale green yellow; but by exposure to light this yellow constituent is decomposed, and the shell becomes a pale flesh-colour from the small residual amount of rufous ooxanthine. Fresh-laid eggs of *Tinamus solitarius* are of nearly the same deep green as those of the Emu; and the long-kept eggs of *Tinamus robustus* are of fine blue, as though in some species there were very little rufous ooxanthine, and the colouring, as in the case of the Emu, due to a mixture of oocyan and yellow ooxanthine. It will thus be seen that all the various peculiar tints can be explained by the presence of a variable quantity of rufous ooxanthine.

I have carefully examined the spectra of many other eggs which appeared at all likely to contain rufous ooxanthine, but have not yet seen any facts which seem to indicate that it occurs in any other group of birds than the Tinamous, unless, indeed, it be in the case of the eggs of *Casuarius bennettii* and *C. australis*. If further examination should confirm these conclusions, it appears to me that the facts will be of much interest in connection with comparative physiology, as showing that, to a limited extent, even in the case of birds' eggs, there is a connection between the general organisation of the animals and their coloured secretions, since, as will be seen, such a well-marked group of birds as

the Tinamous appears to be equally well distinguished by the formation of a special colouring-matter in the egg.

Connection between the Colouring-matters of Eggs and other Organic Products.

It would obviously be very interesting to learn what connection there is between the various colouring-matters described in this paper and substances met with elsewhere. Perhaps further inquiry may lead to the discovery of some of them in other situations; but, with the exception of lichnoxanthine and the pigmentum nigrum, I have not been able to identify them with confidence except in the shells of birds' eggs. The spectra of oorhodeine are so well marked that there could be no difficulty in recognising a comparatively small quantity; and yet no trace can be detected in feathers whose general colour is practically identical with that of birds' eggs coloured with oorhodeine.

In considering the relation between the coloured substances in birds' eggs and other natural or artificial products, we are at once brought face to face with a branch of inquiry which seems to promise most valuable results, but is now so much in its infancy that the conclusions can only be looked upon as very plausible. In a paper recently read before the Royal Microscopical Society* I have shown that in some cases it is certain, and in others probable, that when a coloured constituent is common to a number of distinctly different compounds these may and do generally give spectra which are most intimately related in the *ratio* of the *wave-lengths* of the centres of their absorption-bands, but the *actual* wave-lengths differ in the different spectra. We may perhaps better understand the facts by supposing that when a substance combined with the coloured constituent is replaced by some other, the general *shape* and constitution of the ultimate molecules is so slightly changed that the general character of the spectrum is the same, but the *size* of the molecules so far altered that they are put into relation with waves of light of a different length. It appears to me, therefore, that when we meet with two substances which give almost exactly the same spectra and are changed in the same manner by the addition of reagents—in fact differ from one another only in the *numerical values*, and not in the *relations* of the wave-lengths of the bands or in any other essential particular—we may look upon it as very probable that there is some important chemical or physical relation between the two compounds.

* Monthly Microscopical Journal, vol. xiii. p. 198.

Since writing the above-named paper I have met with a number of facts which make me more and more convinced of the truth of this supposition. I have found several excellent cases in which spectra are closely alike in every particular except in the exact wave-length of the centres of the bands, and though the two substances differ most materially in other characters, it is easy to prove that they are very closely related, and perhaps yield the same product when decomposed by the action of certain reagents. The difference of a few millionths of a millimetre in the wave-length of the bands, which a superficial observer might overlook, may thus serve to teach us far more important facts than if the spectra had been entirely different; and is a good illustration of the importance of minute accuracy in scientific investigation, and of looking upon nothing as accidental and insignificant.

A striking example of such a relation is the connection between the spectra of oorhodeine and of the product of the decomposition of the red colouring-matter of blood by strong sulphuric acid, discovered and described by Thudicum.* When in a nearly neutral state, dissolved in alcohol, it gives a spectrum of exactly the same character as that of oorhodeine in the same physical condition; and on adding a small quantity of a strong acid to both they are both changed in the same manner, and give new spectra which are also of exactly the same character. The spectra of their neutral modifications and also those of the very acid solutions, have most remarkable and unusual peculiarities, quite unlike those of any other substances; and therefore one cannot, I think, attribute the resemblance to mere accident. The agreement is so close that a superficial observer might easily be led to conclude that they were absolutely identical, and that oorhodeine was merely Thudicum's cruentine; but when the spectra are compared together side by side with a suitable instrument, it may be seen that although the number, relative intensity, and relative position of the bands, both in a neutral and acid condition, are exactly the same, the *position* of the band is *not* the same. The difference between the spectra is exactly like what is so often seen on comparing together the spectra of the same substance dissolved in different liquids; but this explanation will not apply in this case, because I find that the position of the bands in cruentine does *not vary* with the nature of the solvent, and the difference between

* Tenth Report of the Medical Officer of the Privy Council, p. 227.

its spectra and those of oorhodeine occurs when they are both dissolved in the *same* solvent. It is more analogous to the connection between the two kinds of hæmoglobin, described in my paper on the evolution of that substance, published in the current number of the "Quarterly Journal of Microscopical Science," which can be proved to contain the same hæmatin united with a different albumenoid.

In order to show the nature of the relation of the spectra, I subjoin the following tables, giving the position of the centres of the absorption-bands in millionths of a millimetre of wave-length.

TABLE 1.—*Dissolved in nearly neutral Alcohol.*

Oorhodeine	630	602	578	539	504
Cruentine.	623	596	572	534	500
Difference . .	7	6	6	5	4

TABLE 2.—*Dissolved in Alcohol with strong Acid.*

Oorhodeine . . .	604	580	557
Cruentine	598	574	552
Difference . .	6	6	5

Though I feel much tempted to enter further into the purely physical part of the question, it will, I think, be better to confine myself to what bears more directly on zoological facts. According, then, to the above-described general principles, these facts lead us to conclude that oorhodeine is in some way or other closely related to cruentine, but not identical with it, as shown not only by the well-marked difference in the spectra, but also by the difference in their solubility and power of resisting the decomposing action of powerful reagents.

In the present state of our knowledge the most plausible explanation of all the facts is that perhaps oorhodeine and cruentine contain some common coloured radical of the same chemical or physical constitution, combined with some other substance which is itself colourless, and that this second constituent is not the same in oorhodeine as in cruentine, but differs sufficiently to modify the general properties and to slightly alter the size of the ultimate molecules, so as to cause them to be related to waves of light of a little different length. Assuming this principle to be true, the facts lead to the conclusion that the oorhodeine of birds' eggs is derived from the red colouring-matter of the blood, not by

any mere mechanical exudation, but by some unknown physiological process of secretion, which breaks up the highly complex molecule of hæmoglobin into one which can be formed artificially by heating it with strong sulphuric acid; but in the living organism it combines with a second substance differing from that with which it combines when the change is effected by the action of hot strong sulphuric acid. Whether this view of the subject be in all respects true or not, it at all events appears to me very plausible and well worthy of further examination, as pointing to the source of one of the most important colouring-matters of birds' eggs.

Relations of the Oocyans.

In their normal condition the fæces of man, and probably those of many other animals, contain a yellow colouring-matter, which by oxidisation yields a substance closely related to, if not identical with, a product of the oxidisation of the bilirubin of bile described by Jaffé* and by Heynsius and Campbell.† When extracted from fæces by alcohol without contact with the air, it gives a spectrum which cuts off the blue end without any definite band; but when exposed to the air, or treated with some oxidising reagent, the solution becomes orange-coloured, and the spectrum shows a well-marked, dark, moderately broad absorption-band between the blue and the green, having its centre at wave-length 495 millionths of a millimetre. The addition of an excess of ammonia immediately removes this band without producing any well-marked change in the colour. Now I find, on comparing this substance with the product of the oxidisation of the two species of oocyan, which gives the spectrum shown by Fig. 2, that there is a close agreement in general characters, but yet a well-marked difference. The band in the product from the oocyans is about 5-4ths the breadth; and its centre is a little farther from the blue end, being at wave-length 497; and caustic potash does not develop any band as in the other substances. On the whole, then, if we follow the same line of reasoning as that adopted in the case of oorhodeine, we are led to conclude that the product of the oxidisation of the two kinds of oocyan is in some way connected with a product of the change and oxidisation of the colouring-matter of bile; and thus we may perhaps be justified in concluding that there is

* Virchow's Archiv., vol. xlvii., p. 262.
† Pfluger's Archiv., vol. iv., p. 520.

some chemical relation between the oocyans and bile. Bilirubin can indeed easily be converted by oxidisation into a blue substance; but this differs entirely from either of the oocyans, both in its spectrum and in the character of the products of its decomposition. The residual bile-product found in fæces is in all probability a representative of a much further stage of change than to the oocyans; and if it could give rise to them it would be by a process of integration, which is not at all likely. On the whole their connection with bile is as if we had two parallel series of products depending on two distinct physiological processes—one in the liver giving bile, and the other in the oviduct giving rise to egg-shell pigments.

The application of the various methods and general principles described in this paper furnishes us with a very wide field for inquiry. It may safely be said that scarcely anything is yet known compared with what remains to be learned. The very foundations of the science require to be laid, and the full significance of the various facts determined, since optical facts have outstripped chemical knowledge. I however trust that the general outline I have given of this particular branch of inquiry will serve to indicate what kind of results we may hope to obtain, and to show that such investigations may throw an unexpected light on many interesting biological problems.

VI. THE EARLIEST MEDICAL WORK EXTANT.

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REFERRING to my "Outlines of a Bibliography of the History of Chemistry,"* Mr. G. F. Rodwell, author of "The Birth of Chemistry," has expressed the hope that in the progress of Egyptian discovery valuable information in regard to the history of chemistry may be brought to light. This hope has been in some measure realised by the appearance of a fac-simile of an Egyptian medical treatise written in the 16th century B.C., which when fully deciphered will undoubtedly prove of immense value to the historian and to the student of science. Though strictly a medical work, it reveals much relating to ancient

* "The Chemical News," vol. xxxii., pp. 36, 56, 68.

Egyptian domestic life, and is said to be the largest, best preserved, and most legible text in the language of hieroglyphics.

The title translated is as follows :—

Papyrus Ebers, the Hermetic Book of Medicines of the Ancient Egyptians, in Hieratic Writing. Published, with Synopsis of Contents and Introduction, by George Ebers. With a Hieroglyphic-Latin Glossary by Ludwig Stern. Under the Patronage of the Royal Bureau of Education in Saxony. Leipzig: William Engelmann, 1875. 2 vols. Folio.

The papyrus of which this work is a fac-simile reproduction was discovered by the archæologist Ebers, during his visit to Egypt in the winter of 1872-3. Ebers and his friend Stern were residing at Thebes, collecting archæological data, and there became acquainted with a well-to-do Arab from Luxor, who brought to them for sale a wooden image of Osiris and a papyrus of no especial value. Suspecting that the Arab was holding in reserve objects of greater interest, Ebers offered him a considerable sum for any superior specimens in his possession. This induced the Arab to return on the following day, bringing with him a metallic case containing a papyrus roll enveloped in mummy cloths. Ebers immediately perceived he had a prize, but was unable to command the large sum of money demanded for it until provided with the means through the liberality of a German gentleman, Max Günther, travelling in that vicinity. According to the Arab's account, the papyrus had been discovered fourteen years previously, by a man since dead, between the bones of a mummy in a tomb of the Theban Necropolis.

Ebers hastened back to Leipzig with his precious roll, and deposited it for safe keeping in the University Library of that city. And now, with the co-operation of an enterprising publisher and the assistance of royal patronage, he places it at the disposal of the civilised world, by reproducing it in these handsome volumes.

The papyrus as received by Ebers consisted of a single solidly-rolled sheet of yellow-brown papyrus of finest quality, three-tenths of a metre wide, and a little more than twenty metres long. It formed one enormous book, but was divided into 110 pages, which were carefully numbered. For purposes of preservation and exhibition in convenient form it has since been cut into several lengths. The writing, which is exceedingly clear and regular, is partly in black and partly in red ink, the latter occurring at the heads of sections and in the expression of weights and measures. The characters

are known as Hieratic, being a cursive form of the hieroglyphic method of writing, and bearing the same relation to the latter that our ordinary written hand does to printed characters. Hieratic script resulted from attempts to simplify the forms and outlines of the ideographic characters employed in hieroglyphic writing, which is essentially a combination of picture-writing with a phonetic system. Hieroglyphics in ancient Egypt were the written language of the people, and Hieratic writing was chiefly confined to the sacerdotal caste.

The Papyrus Ebers is so marvellously well preserved that not a single letter is lacking in the entire roll. The material of the papyrus itself, the inner bark of *Cyperus papyrus*, was examined by Professor Schenck, Professor of Botany in the University of Leipzig, who established its identity with that of similar rolls, and pronounced it of remarkably good manufacture.

The age of the manuscript was determined by a consideration of three points:—1. Palæographic studies of the form of the written characters; 2. Occurrences of names of Kings; 3. Examination of a calendar which occurs on the back of the first page. These data enable Ebers to assign the writing to the middle of the 16th century, or more precisely 1552 B.C. Accepting this date—and it has been established beyond reasonable doubt—the writing was prior to the exodus of the Israelites: in fact, according to the commonly received chronology, Moses, in 1552 B.C., was just 21 years of age.

The authorship of this ancient treatise is not revealed, but it bears internal evidence of being one of the six Hermetic Books on Medicine named by Clement of Alexandria (200 A.D.) The Egyptian priests, who were also the physicians, in order to give greater authority to their writings, were wont to ascribe them to their gods, and their codified medical knowledge was generally ascribed to the god Thuti (or Thoth). In proof of this, Ebers quotes the following passage from page 1, lines 8 and 9, of the papyrus in question:—

"Ra pities the sick; his Teacher is Thuti, who gives him speech, who makes this book and gives the instruction to scholars and to physicians in their succession."

This god Thuti, also written Thoth and Taant, is the famous Hermes Trismegistus of the Greeks, the same who was regarded by the alchemists of the Middle Ages with superstitious reverence as the Father of Alchemy. However this may be, historians accord in representing Hermes

as the inventor of arts and sciences, and that he first taught the Egyptians writing, invented arithmetic, geometry, astronomy, and music; gave laws to the people, and regulated their religious ceremonies. At the time of Jamblichus, A.D. 363, the priests of Egypt showed 42 books which they attributed to Hermes (Thuti). Of these, according to that author, 36 contained the history of all human knowledge; the last six treated of anatomy, of disease, of affections of the eye, instruments of surgery, and of medicines. The Papyrus Ebers is indisputably one of these ancient Hermetic works. A study of the synopsis of the contents given in part below will justify this belief.

The recipes and prescriptions contained in this treatise are evidently collected from various sources, some of them being quoted from still more ancient writings. It bears internal evidence of having been used in the healing art, for the word "good" occurs in the margin in several places, written in a different handwriting from the body of the work and with lighter coloured ink. Ebers thinks the compilation was made by the College of Priests at Thebes, basing his conjecture partly on the locality in which it was discovered.

Ebers gives a synopsis of the contents of the entire work and a literal translation of the first two pages of the roll, reserving a fuller translation with commentary for a future publication. A hieroglyphic translation of a portion of the Hieratic manuscript also accompanies the plates; the latter, 107 in number, are faithful and beautiful reproductions of the original papyrus in the same yellow-brown colour. The second volume contains a hieroglyphic-Latin glossary by Stern and the remainder of the plates. Before proceeding to give details of its contents, one more peculiarity is worth mentioning. Though the pages are carefully numbered, the figures 28 and 29 are omitted, while the text is continuous. Ebers conjectures that the writer either accidentally forgot his count or abstained from using these numbers for superstitious reasons.

As already remarked, the work is divided into chapters or sections. A fair insight into the character of the treatise may be obtained from the selected headings of sections and extracts here following:—

Headings of selected chapters. The numbers refer to the pages of the papyrus:

1. Of the preparation of medicines.
25. Of salves for removing the *uhan*.
47. Catalogue of the various uses of the *Tequem* tree.

48. Medicines for alleviating the accumulation of urine and diseases of the abdomen.

55. The book of the eyes.

65. Medicaments for preventing the hair turning grey and for the treatment of the hair.

66. Medicines for forcing the growth of the hair.

79. Salves for strengthening the nerves and medicines for healing the nerves.

85. Medicine for curing diseases of the tongue.

89. Medicines for the removal of lice and fleas.

91. Medicines for ears hard of hearing.

99. The Secret Book of the Physician. The science of the beating of the heart and the knowledge of the heart as taught by the priestly physician Nebsecht.

Verily "There is no new thing under the sun." Chapters 65, 66, 79, and 89 show that hair invigorators, hair dyes, pain-killers, and flea powders were desiderata 3400 years ago.

Ebers encountered immense difficulties in the work of deciphering this papyrus on account of the large number of technical terms; as an example of the obstacles met, he gives the following literal translation of a diagnosis beginning on plate xxxvi., line 4:—

"Rules for the *re-het*, that is, suffering in the pit of the stomach (pylorus or cardia.) When thou findest anybody with a hardening of his *re-het*, and when eating he feels a pressure in his bowels (*chet*), his stomach (*het*) is swollen, and he feels ill while walking, like one who is suffering from heat in the back, *tau nu peht*, then look at him when he is lying outstretched, and if thou findest his bowels hot and a hardening in his *re-het*, then say to thyself this is a liver complaint, *sepu pu n merest*. Then make thyself a remedy according to the secrets in botanical knowledge from the plant *pa chestet* and from scraps of dates. Mix it and put it in water. The patient may drink it on four mornings to purge his body. If after that thou findest both sides of his bowels (*chet*), namely, the right one hot and the left one cool, then say of it: That is bile. Look at him again, and if you find his bowels entirely cold, then say to thyself: His liver, (?) *merest*, is cleansed and purified; he has taken the medicine, *sep nef sep*, the medicine has taken effect."

In view of the direction to look at the patient "when lying outstretched," it is curious to note that (according to Dunglison) the priestly physicians of Egypt are said by Diodorus to have formed their diagnosis principally on the position which the patient assumed in bed.

The following is the translation of the first four lines of Plate I. :—

"The book begins with the preparation of the medicines for all portions of the body of a patient. I came from Heliopolis with the Great Ones from *Het aat*, the Lords of Protection, the masters of Eternity and Salvation. I came from Sais with the Mother-goddesses, who extended to me protection. The Lord of the Universe told me how to free the gods from all murderous diseases."

The work abounds in prescriptions, of which the following are samples :—

Beginning of the Book of Medicines. To remove illness from the stomach. Rub up the seed of the *Thehui* plant with vinegar and give the patient to drink.

The same for sick bowels.

Caraway seed	1-64 dram.
Goose fat	1-8 dram.
Milk	1 tenat.

Boil, stir, and eat.

The same :

Pomegranate seed	1-8 dram.
Sycamore fruit (?)	1-8 dram.
Beer	1 tenat.

Treat as above.

In the original the arrangement of the substances and quantities in two columns is the same as here given. The weights are written in red ink. Other prescriptions contain reference to pills made by mixing certain substances with honey and rolling them into little balls. The weights and measures in this unique work deserve a longer notice than space will permit. Certain characters with dots above them represent weights, and a series of special signs indicate measures of volume. The unit of weight employed is believed by Ebers to bear a close relation to the later Arabic dirhem, or dram, which is equivalent to about 48 English grains. But owing to the smallness of the quantities given in the prescriptions, the unit is probably double the drachm in value. This unit is represented in hieroglyphics by a spindle-shaped figure, and divisions of this unit into eighths, sixteenths, thirty-seconds, and sixty-fourths were indicated by arbitrary dots and other characters placed beneath the sign of the unit, in certain positions; the fractions 1-8, 1-16, 1-32, 1-64 always recurring, and

1-16 predominating; a quarternary arrangement which was superstitiously regarded as beneficial.

The unit of volume is thought to be the *tenat*, which is equivalent to six-tenths of a litre. This unit and its subdivisions are represented in the hieratic script by arbitrary signs. When equal parts of the components of a prescription are taken, the fact is indicated by a light short vertical dash placed opposite each substance.

Ebers states in his preface that notwithstanding there are to be found in this wonderful work many incantations and conjurations from which the priestly physicians could not abstain, still there is no hocus pocus nor gibberish in it. On the contrary, it shows that it was possible to write in the sixteenth century B. C. complex recipes, and that they understood how to administer with care the medicines prescribed. Moreover, sorcery was forbidden in the ancient times in the strongest manner, and the alchemistic magi were punished in the reign of Rameses III. with death. The art of the physician was lost in the post-Christian era; science became more and more tinged with magic, and was gradually obscured and degraded by it.

NOTICES OF BOOKS.

The Movements and Habits of Climbing Plants. By CHARLES DARWIN, F.R.S. Second Edition. London: Murray.

EVEN in the woodlands and hedge-rows of so-called temperate regions, climbing plants form a striking feature. Their graceful forms, the ease with which they are adapted to decorative purposes, and a certain weird character, which they seem to share with the serpent tribe, appeal at once to our æsthetic and imaginative faculties. But on a closer scrutiny we find that they afford at least an equal scope for the spirit of scientific inquiry. The first point which strikes even the cursory observer is the diversity of means by which the common object of deriving support from other plants or from inanimate substances is attained. We see plants with long flexible shoots which merely scramble over and through bushes, supporting themselves by their side-twigs, their leaves, or their prickles. Familiar instances of this may be found in brambles. These scramblers stand on the debateable land of climbing plants. If support is to be found they accept it in a rough way. But if not, they form independent bushes. Then we find root-climbers, of which the ivy may serve as an illustration. From its twigs it sends out broad, flattened roots, which attach themselves to every crevice and irregularity of the surface up which the ivy is climbing. Hence it is admirably adapted for covering the faces of rocks, walls, or the trunks of thick trees. Next come the true twiners, which twist spirally around any object which they are able to grasp, but are quite unfit to cling to a flat surface. The hop and the common bindweed, as well as its garden congener the "morning glory," are types of this class. Lastly, we have plants which, like the vine and the pea, climb by means of special organs for laying hold of any suitable object. The next point which must have struck every observer even slightly versed in plant-lore is that climbers do not form or belong to any one botanical order or group of orders, but appear scattered through the whole vegetable kingdom.

Such, we may say, was the state of popular knowledge on the subject when Mr. Darwin entered upon the researches which form the matter of the work before us. He does not, however, profess to be the first scientific investigator of the phenomena presented by climbing plants. When his observations were more than half completed he learned that the "spontaneous revolutions of the stems and tendrils of climbing plants had been observed by Palm and Hugo von Mohl" as far back as 1827, and had been again investigated by Dutrochet in 1843. We may

here remark that the difficulty of finding whether a given subject has been already investigated is greater in Natural History than perhaps in any other branch of science. Important observations in zoology and botany may be found, not merely in the majority of scientific and literary periodicals, but even in sporting and political organs.

It must not, however, be supposed that Mr. Darwin's sole merit in this matter consists in verifying previous researches and in presenting them in a form accessible to the English reader. He tells us, with perfect justice :—" I believe that my observations, founded on the examination of above a hundred widely distinct living species, contain sufficient novelty to justify me in publishing them."

Twining plants form, it appears, the largest subdivision of the climbers, and represent, according to Mr. Darwin, the primordial and simplest condition of the class. If a young hop-shoot be observed as it rises from the ground, the first two or three joints are straight, and remain stationary, like the shoot of a non-climbing tree. The next joint, however, when still quite young, bends to one side, and moves slowly round to all points of the compass, travelling with the sun. In hot weather, if the plant is in vigorous health, each revolution is completed in two hours and about eight minutes. As the plant grows up the older joints lose this property, but the three top joints always continue to rotate. If the shoot is left free it describes a circle of about 19 inches in diameter. Another twiner, the *Ceropegia Gardnerii*, revolves in a direction opposite to the sun, and describes a circle of 62 inches in diameter. When one of these revolving shoots encounters a stick it twines round it in a spiral form. The thickness of the object found by a shoot is a very material point. The common nightshade (*Solanum dulcamara*) can twine only around such stems as are at once thin and flexible. The only native English twiner which can clasp trees is the honeysuckle, which Mr. Darwin has found twining up a young beech tree $4\frac{1}{2}$ inches in diameter. In a room lighted on one side *Phaseolus multiflorus* could not ascend posts of from 3 to 4 inches in diameter. In the open air it could twine round supports of this thickness, but failed ascending one of 9 inches. In South Brazil F. Müller saw a tree about 5 feet in circumference spirally ascended by a plant belonging to the Menispermaceæ. Mr. Darwin very aptly remarks that in cold climates it would be " injurious to the twining plants which die down every year if they were enabled to twine round trunks of trees, for they could not grow tall enough in a single season to reach the summit and gain the light." Twining plants with very long revolving shoots are not necessarily able to ascend thick supports, their great length and power of movement merely aiding them in finding a distant object up which to climb. The rate of revolution in all the plants observed by Mr. Darwin was merely the same by day as

by night, whence he infers that the action of the light is confined to retarding one semicircle and accelerating the other, not greatly modifying the speed of the whole revolution. It has actually been found that in *Ipomœa jucunda* the semi-circle from the light takes $5\frac{1}{2}$ hours, whilst the semi-circle towards the light is effected in 1 hour. In most twiners the branches, however numerous they may be, all go on revolving together. In *Tamus elephantipes* only the side branches twine and not the main stem. In a certain climbing asparagus the case was reversed. A plant of *Combretum argenteum* made a number of short, healthy shoots, which showed no signs of revolving, but at last it put out from the lower part of one of its main branches a thin sheet, 5 or 6 feet in length, which revolved vigorously and climbed. *Polygonum convulvulus*, according to Palm, twines only during the middle of the summer, but in autumn, even if growing vigorously, shows no tendency to climb. Three vegetable species, —two *Ceropegias* and *Ipomœa argyrœoides*—in their dry home in South Africa grow erect and compact, but seedlings raised near Dublin, presumably in a conservatory, twined up sticks from 6 to 8 feet in height. On these significant facts Mr. Darwin thus comments:—"There can hardly be a doubt that in the drier provinces of South Africa these plants have propagated themselves for thousands of generations in an erect condition: and yet they have retained during this whole period the innate power of spontaneously revolving and twining whenever their shoots become elongated under proper conditions of life."

But we must now turn from the twiners to those plants which climb by means of prehensile organs, possessing a certain sensibility or irritability. The simplest and least developed of this class are the leaf climbers, which seize hold of any point of support either by the foot-stalks of their leaves, or by a prolongation of the midrib. Here, also, there is the power of revolving at various rates. But though no very sharp line of demarcation can be drawn between the twiners and the leaf-climbers, and some few of the latter "can ascend by twining spirally round a support;" yet the general object of the revolving motion is here to bring the foot-stalks or the prehensile tips of the leaves into contact with surrounding objects. The leaves are sensitive to a touch and to continued pressure even when very slight. Leaf-climbing may be easily understood by observing the species of *Clematis* and *Tropœolum*, including the common nasturtium. This plant, if it meets with a string or a thin twig, casts a hitch around it with one of its leaf-stalks, and thus secures a point of support.

More highly specialised are the plants which climb by the aid of tendrils, which Mr. Darwin defines as "filamentary organs, sensitive to contact, and used exclusively for climbing." These organs "are formed by the modification of leaves with their foot-stalks, of flower-peduncles, branches, and possibly stipules. In

this group, which includes the vine, the pea, and a number of Bignonias, the climbing organisation reaches its highest development. The twiner, in ascending a tree by its spiral folds, must, in order to reach the light above, describe a line very much longer than the perpendicular height to which it rises. Consequently it is compelled to expend a relatively large amount of matter. But the tendril-climber can ascend nearly in a straight line. The action of the tendrils is very curious. They revolve, and the shoot of the plant not unfrequently revolves also. If they touch any object they immediately begin to coil round it if thin enough, and become at the same time very much thicker and stronger. Tendrils which do not succeed in clasping anything generally wither and fall off. If the object found is too thick to be clasped, the points of the tendrils in some plants "exhibit a singular habit, which in an animal would be called an instinct." They continually search for any little chink or hole into which they may insert themselves. In other cases the ends of the tendrils are converted into flat discs, which are pressed close to the surface up which the plant is climbing. *Bignonia Tweedyana*, which Mr. Darwin has carefully studied, "combines four different methods of climbing generally characteristic of different plants, namely, twining, leaf-climbing, tendril-climbing, and root-climbing."

Among true root-climbers we fine a curious phenomenon—*Ficus repens*—a plant which creeps up a wall exactly like ivy, secretes from its rootlets an adhesive fluid, by which they are cemented to the wall or rock. This fluid was found to be slightly viscid, and on exposure to the air did not dry up. From experiments and observation made it would appear that the rootlets "first secrete a slightly viscid fluid, subsequently absorb the watery parts, and ultimately leave a cement." This appears to be a modified form of caoutchouc—a substance in which the genus *Ficus* is well known to abound.

A careful consideration of climbing plants can scarcely, in our opinion, fail to furnish evidence in favour of the doctrine of evolution. If we place any plant in the open ground, freely exposed to light and air from every side, we find it generally assume a compact, rounded habit; but if we set it where light and air are more or less cut off, as near lofty trees, among bushes, or in a thick wood, then two cases are possible. If the soil is poor, and if moisture is deficient, the plant will languish or even die; but if the earth be fruitful and moisture abundant, it will shoot out long, slender stems, seeking to win its way to the light. As gardeners and farmers often say, it will be "drawn" by the overtopping objects. Thus, then, the very circumstances which would render it necessary or desirable for a plant to climb, enable it, at any rate, to take the first step, by becoming more slender, longer in its joints, and more flexible.

If, as seems not unlikely, from several facts detailed in the work before us, there is a latent tendency to revolve in the shoots of all plants, this very attenuation and elongation will remove what was before a hindrance, and the power of twining may thus be gradually developed. Those forms which thus became climbers would in certain situations enjoy a great advantage over their rivals. We find further that the twining faculty appears in plants in many different grades. In some it is highly developed, in others dormant. There are cases where it appears to have been scarcely attained, and others in which it is becoming obsolete. From the twiner to the leaf and tendril-climbers the way is paved by small gradations. We cannot fail to recognise that the latter especially must have nearly the same advantage over twiners as these have in turn over plants unable to climb at all.

We cannot better conclude this brief and necessarily imperfect survey of a profoundly interesting subject than by quoting a portion of the final paragraphs of the work before us:—"When we reflect on the wide separation of these (climbing plants) in the series, and when we know that in some of the largest well-defined orders, such as the Compositæ, Rubiaceæ, Scrophuliariaceæ, &c., species in only two or three genera have the power of climbing, the conclusion is forced on our minds that the capacity for revolving, on which most climbers depend, is inherent, though undeveloped in almost every plant in the vegetable kingdom.

It has often been vaguely asserted that plants are distinguished from animals by not having the power of movement. It should rather be said that plants acquire and display this power only when it is of some advantage to them; this being of comparatively rare occurrence as they are fixed to the ground, and food is brought to them by the air and rain."

A Course of Practical Instruction in Elementary Biology. By T. H. Huxley, LL.D., Sec. R.S., assisted by H. N. Martin, D.Sc. London: Macmillan and Co.

This work is arranged upon a somewhat novel plan. The author describes in succession yeast, protococcus, the proteus animalcule, bacteria, moulds, stone-worts, the bracken-fern, the bean-plant, the bell-animalcule, the fresh-water polypes, the fresh-water mussel, the fresh-water crayfish and the lobster, and, lastly, the frog. After the description follows in each case a section headed Laboratory Work, and containing instructions for the practical examination of the plant or animal in question. By way of a specimen of the task thus set the student, we insert an abridgment of the "Laboratory Work" on the *Amœba*:—"Place a drop of water containing *Amœbæ* on a slide, cover with a cover-glass, avoiding pressure, and search over with a

¼ inch obj.; having found an *Amœba*, examine with a higher power. Note—

1. *Size.*
2. *Outline.*
3. *Structure.*
    a. Outer hyaline border (*ectosarc*) tolerably sharply marked off; granular layer (*endosarc*) inside this, gradually passing into a more fluid central part.
    b. *Nucleus* (absent in some specimens), a roundish, more solid looking particle which does not change its form.
    c. *Contractile vesicle;* in the ectosarc note a roundish clear space which disappears periodically and after a time reappears; its slow diastole — rapid systole. Not present in all specimens.
    d. *Foreign bodies* (swallowed); diatom cases, *Desmidiæ*, &c.
4. *Movements.*
    a. Watch the process of formation of a *pseudopodium.*
    b. Locomotion.
    c. If the opportunity presents itself watch the process of the ingestion of solid matters.
    d. Observe movements on hot stage; as the temperature approaches 40° C. they cease.
    e. Effects of electrical shocks on the movements.
5. *Mechanical analysis.* Crush. The whole collapses except the nucleus, and even that after a time disappears.
6. *Chemical analysis.* Treat with magenta and iodine. The whole stains, leaving no unstained enveloping sac. Iodine, as a rule, produces no blue colouration; if blue specks become visible it is probable that the starch they indicate has been swallowed.
7. Look for encysted specimens, and for specimens which are undergoing fission.
8. Another form of *Amœba* is sometimes found much less coarsely granular, having no well-defined ectosarc and endosarc, and having much longer, more slender, and pointed pseudopodia."

It must be admitted that a systematic course of such work will not only fix the characteristics of an animal or plant much more clearly and enduringly in the memory of the student than any amount of mere reading, but will be an admirable mental discipline. He who cannot thus learn the precious art of observation may dismiss the subject as outside his capacity.

It will be perceived that Professor Huxley regards the "study of living bodies as really one discipline, which is divided into zoology and botany simply as a matter of convenience." He holds that "the scientific zoologist should no more be ignorant of the fundamental phenomena of vegetable life than the scientific botanist of those of animal existence." This view can scarely be disputed. There is much that is essentially common to plants and animals, and the tendency of modern research

goes more and more to remove those arbitrary landmarks once set up between the two organic kingdoms. No one now considers the lowest animal as one stage above the highest plant. At the same time, we must be cautious. Whilst all that is common to the two great divisions of the organic world should be deemed equally necessary for the botanist and the zoologist, and while the methods employed by both are identical, and while neither can afford to be ignorant of the main results of the other, few persons have the time and the opportunities to enter into the details of both. The attempt to be great in many departments of knowledge leads too commonly to failure in all. The man who aims at being "good all round" in science loses his way even more signally than the "jack of all trades" who is "master of none." It need scarcely be said that the paltry jealousies which formerly existed between the respective students of animal and vegetable life have passed away. There is, we believe, only one living person—a member, if we mistake not, of the "Victoria Institute"—who has displayed his bad logic and worse taste by likening botanists to greengrocers.

The road to a sound and thorough knowledge of zoology and botany obviously lies, according to Professor Huxley, through morphology and physiology. It seems strange to us, in these days to refer to the works of Swainson—who within the memory of many still living was considered the most philosophic naturalist of the day—to find him proclaim these disciplines unnecessary, if not actually injurious to the student of animated nature. It is quite true that a man totally ignorant both of morphology and physiology may identify species, note their localities and observe their habits, and in this way may do science good service. But if we wish to study thoroughly any one animal, or class of animals, we shall find ourselves baffled and bewildered at every step if our knowledge extends no farther than its surface. Swainson considered it absurd to suppose that we could not understand an animal or plant without taking it to pieces. But neither animals nor plants exist for us to classify. Nature has not labelled and pigeon-holed them for our convenience. If we wish to understand and arrange them in any rational manner, we need every help that their internal as well as their outward structure can give us. Few persons have been foolish or eccentric enough to study Natural History from books only. Almost every zoologist, every botanist, collects specimens, and it is fortunately hard to do this without doing something more, without noting to some extent the economy and the localities of the specimens collected. But too many are utterly wanting in that solid foundation which Professor Huxley seeks to supply in the work before us. Even the determination of species is effected by many merely by *rote* without any knowledge of principles. We have been present at a botanical meeting in the north of England conducted in this spirit. A promiscuous assemblage of plants had been brought

in and were laid at the head of the table. The president took up one, pronounced its generic and specific name, and handed it to his right-hand neighbour. He in turn examined it, if he thought needful, and passed it on, repeating the name, which, by the time the plant had travelled round the table was often strangely travestied. In this manner numbers of men learn by *rote* the names of all ordinary species in the British flora, but if they were asked the reason for referring any particular plant to some given genus they would be unable to reply. Precisely the same practice exists as regards insects. They learn that such and such British butterflies bear the generic name *Vanessa*, but if shown an exotic *Vanessa* they would be at a loss where to place it. Surely no branch of Natural History should be left to rest upon such a foundation.

Theory of Heat. By J. Clerk Maxwell, F.R.S., &c. Fourth Edition. London: Longmans, Green, and Co.

This book, though it may be pronounced indispensable for every student of physics, is not, in the ordinary sense of the word, a manual of the science of heat. Its object, as declared in the preface, is "to exhibit the scientific connection of the various steps by which our knowledge of the phenomena of heat has been extended." For an account of many important experiments on the effects of heat, which could not have been introduced without extending the work to an inconvenient bulk, the reader is referred to other well-known publications.

The most profound interest attaches to the last section in which the distinguished author treats on the "nature and origin of molecules." The molecules of the same substance, he declares, are all exactly alike, but different from those of other substances. There is no regular gradation in their mass from that of hydrogen up to that of bismuth, but they fall into certain classes or species. Unlike the case of plants and animals, it is not possible, Dr. Maxwell maintains, to account for the state of molecules by natural selection. Each individual is permanent; there is neither generation or decay, nor even any difference between the individuals of each kind. Hence the doctrine of evolution is here inapplicable. Hence to account for the equality of molecules in magnitude and in their natural periods of vibration we must either, as the author once expressed this view, declare that they bear the stamp of manufactured articles, or consider them as the primordial materials of the cosmos, the bricks with which its architects—conscious or unconscious, mediate or immediate—have had to work. Neither of these hypotheses is really satisfactory. To pronounce them primordial is something like the explanation a savage might give of the origin of a mountain. To declare them manufactured is merely to elicit further question as to their raw material.

Elementary Treatise on Physics, Experimental and Applied, for the Use of Colleges and Schools. Translated and Edited from Ganot's "Eléments de Physique." By E. Atkinson, Ph.D., F.C.S. Seventh Edition, Revised and Enlarged. London: Longmans, Green, and Co.

The fact that this book, in its English version, has reached its seventh edition since 1863 deprives us of the ordinary scope for criticism. Ganot's work is known and appreciated by teachers and students here as well as on the Continent. The present edition contains seventeen entirely new illustrations, whilst the additions to the text amount to twenty-seven pages. An appendix has also been added containing a series of numerical problems and examples in physics, arranged so as to afford students who have not the advantage of regular professorial instruction a means of testing the accuracy and thoroughness of their knowledge. We have every reason to believe that the present edition of this book will be received even more favourably than the foregoing.

A Dictionary of Science, Literature, and Art. Edited by W. T. Brande, D.C.L., F.R.S. L. and E. (late of Her Majesty's Mint), and the Rev. G. W. Cox, M.A. New Edition. London: Longmans, Green, and Co.

This work is, in point of fact, an encyclopædia, embracing nearly the whole extent of human knowledge. Its total extent being merely three octavo volumes of some nine hundred pages each, minuteness of detail cannot be expected. The distribution of the space at command in a publication of this nature is a matter on which differences of opinion are certain to prevail. The chief attention of the surviving editor has been, perhaps naturally, turned rather to literature and art than to science. The historical, archæological, and mythological portions strike us as having been carried out on a larger scale than other subjects. Technology, chemical, physical, and mechanical, cannot be said to have fared well. Calico-printing, for instance, is despatched in nine lines, the soda-manufacture receives not quite a column, whilst "ballad" enjoys a column and a quarter, and "ballot" a column and a half. It may, however, be considered that those who take a special interest in technological questions will have recourse to Dr. Ure's "Dictionary of Arts," to which the work before us may in some sort be regarded as complementary.

The scientific articles are of very varying degrees of merit. Those treating on biological subjects are perhaps the least satisfactory. Where matters of fact are concerned, the notices are exceedingly brief, and a part of the little space at command is generally spent on the derivation of the *name* where further information on the *thing* named is urgently needed. Important

genera, and even groups of higher rank, are sometimes totally overlooked. The theoretical articles, see *e.g.* "Species," are characterised by a certain feebleness and looseness of thought.

Chemistry and mineralogy are treated with greater ability, or at least with more care, whilst the articles on astronomy and mathematics will, we think, exceed both the wants and the comprehension of all readers except such as have made these sciences the subject of special study.

Having thus pointed out what from our point of view appear as imperfections, we have pleasure in adding that the work contains a vast amount of useful and valuable information. He must be indeed a learned man who in turning over these volumes does not occasionally come upon some fact of which he was before totally ignorant. This, indeed, is the chief use of encyclopædias. They are not intended to make specialists, but to supply that general knowledge without which the most accomplished specialist often finds himself "at sea," and which he has not the time nor the opportunity to seek out in its original sources.

Air and its Relations to Life. By WALTER NOEL HARTLEY, F.C.S. London: Longmans, Green, and Co.

THIS book is, as the title-page informs us, the substance of a course of lectures delivered at the Royal Institution in the summer of 1874. The character of the work is avowedly "light and popular," as its origin necessitates. Still, though scientific technicalities have been as far as possible avoided, there is no room to complain of a want of accuracy. The work is more comprehensive than might perhaps be anticipated from its title. The author treats successively of the proof of the existence of the air, of Priestley's discovery of oxygen, and Lavoisier's discovery of the nature of the air; of the reasons for regarding the air not as a true chemical compound, but as a mere mechanical mixture, and of Tessié du Motay's process for obtaining oxygen from the air. In the second chapter, Mr. Hartley treats of Black's experiments on the carbonating of lime; on the properties of carbonic acid; the presence of aqueous vapour and of ammonia in the atmosphere; the preparation of ozone; on Dr. Angus Smith's method of determining the proportion of carbonic acid in the air; and of the reciprocal action of plants and animals. In the next chapter, we find an account of the means whereby a constancy in the composition of the atmosphere is maintained. This leads to a consideration of the expansion of gases by heat, the intermixture of gases in apparent opposition to their specific gravities, and on gaseous diffusion. This naturally leads up to the subject of ventilation, with the evil effects of foul air, and a

notice of Pettenkofer's recent investigations on "ground-air" and its passage into our dwellings. The remaining two chapters may be considered biological rather than physical or chemical. They treat of spontaneous generation, of minute organisms present in the air, and of the researches of Schwann, Schultze, Schrœder and Dusch, Pasteur, Pouchet, and Bastian. Concerning the conclusions of the latter author, we must call attention to the fact, that however decisively it might be proved that solutions of organic matter subject to temperatures above the boiling-point of water in sealed vessels were subsequently found to contain living organisms, not introduced from the atmosphere, such a result can throw no light on the first appearance of vegetable and animal life. On the primordial globe, before plants and animals existed, there can have been no organic matter. To prove the reality of spontaneous generation, it will, therefore, be necessary to produce low forms of organic life from purely inorganic matter, atmospheric germs being, of course, excluded by precautions similar to those adopted by Pasteur.

In connection with the important subject of ventilation and the influx of ground air into our dwellings, Mr. Hartley gives from his own observation the following interesting fact :—"A remarkable case in a London house has come to my knowledge, which gives a distinct proof of the much greater passage of gases through the walls in winter than in summer. A small room, occasionally used, was noticed sometimes to have an unbearably bad smell ; this was never noticed in summer nor in winter unless a fire was lighted in the room ; the drainage was suspected and examined, but was found perfect, yet here was this extraordinarily foul air making its way into the room whenever the interior was warm and the exterior cold. The cause was a dust-bin built against one of the walls, and the filtration of the air through this and the house-wall into the room." In passing, we may here remark that in all "closetted" towns, the "dust-bin" is an evil with which sanitary reformers scarcely know how to deal. It often contains matter which cannot be forced down the soil-pipe, and which is yet little less offensive than true sewage matters. It is no pleasant thing to have a heap of cabbage leaves, parings of vegetables, oyster-shells, fish-bones, &c., fermenting in close proximity to a dwelling house. Another point which naturally suggests itself in connection with the subject of "ground-air," is the danger which may follow from the adoption of asphalte roadways. These being impervious to air, the sewage gases will take the direction of least resistance, *i.e.*, they will rise up through the earth beneath the houses all the more readily on account of the ascending current of warm air within. A sound layer of asphalte should extend beneath every house, or else our dwellings, as Dr. Richardson proposes in his model city, should be built upon arches, so that the wind may have free play beneath them. Mr. Hartley very pardonably expresses a doubt as to the sanitary value of the

houses built with concrete in iron frames, which may possibly not prove sufficiently porous for the walls of healthy houses. Slag he very rightly condemns as a building material, and gives from Pettenkofer an instance of a house constructed of blocks of this material being permanently and incurably damp.

The author quotes the experiments of Dr. Angus Smith, showing that carbonic acid alone, quite irrespective of organic matter derived from the lungs and the skin, &c., has a bad effect. One part of carbonic acid in 1000 of air occasioned an increase of 18 to 19 respirations per minute, the speed of the pulse at the same time being diminished. We observe the statement made on the authority of Dr. Odling, that for equal illuminating power, candles yield a larger amount of "impurity" than gas. We presume the impurity here meant is carbonic acid, but it must not be forgotten that all ordinary coal-gas contains sulphur compounds, whose products of combustion are far from desirable.

In concluding this necessarily brief examination, we must pronounce Mr. Hartley's book a valuable addition to our popular scientific literature. All persons of decent education, be they young or old, may read it with pleasure and advantage.

The Dictionary of Chemistry and the Allied Branches of other Sciences. By H. Watts, F.R.S. Second Supplement. London: Longmans, Green, and Co.

A Dictionary of chemistry is in its very nature interminable. So rapid is the growth of the science that by the time the compiler has completed his task and reached the end of the alphabet, the crop of new researches which have sprung up in the meantime compel him to resume his pen and to amend, add, and rescind in accordance with the most recent authorities.

This supplement, we are told, only brings the record of chemical discovery down to the end of the year 1872, including some of the more important discoveries which have appeared in 1873 and 1874. Among the chief articles in this volume are papers on the phenols and on sulphur chlorides by Professor Armstrong; on magnetism, Prof. by G. C. Foster; on digestion, gastric juice, muscular tissue, respiration, and urine by Dr. H. N. Martin; on the chemical action of light and on spectral analysis by Professor Roscoe, and on topics belonging to agricultural chemistry by R. Warington. The articles on analysis, chemical action, gases, and many others from the pen of the editor are deserving of notice. The work may be considered as an essential requisite in the library of every chemist.

An Introductory Text-book of Zoology, for the Use of Junior Classes. By H. Alleyne Nicholson. Second Edition. Edinburgh and London: W. Blackwood and Sons.

A Work which, in thecompass of some two hundred pages, undertakes to give a general survey of the whole animal kingdom cannot be expected to enter into details. But this limited amount of space has been very judiciously utilised. The author, we are happy to find, devotes more attention to the "invertebrate animals than has usually been the case in introductory works on zoology." He very justly remarks that "the vertebrate animals are of no greater *zoological* value or interest than any other of the primary divisions of the animal kingdom," and adds the very important consideration that "any practical work undertaken by beginners in zoology will almost certainly lie in the department of the Invertebrata," a department, we must never forget, which embraces by far the majority of animal species, which presents the greater amount of unsolved questions, and which includes our most formidable enemies.

There are in the work certain traces of Cuvierian inspiration. Man is still placed by himself in the order "Bimana," though the author admits that the "purely anatomical distinctions between man and the other Mammals are by no means so striking as might have been anticipated." From a consideration of this passage, and of another in the introduction, it would seem that Dr. Nicholson upholds the doctrine of a marked distinction between man and the rest of the animal creation.

Another Cuvierian feature is that in a general arrangement in which man comes last, the Mollusca are placed before the Annulosa. Within the class Insecta, also, the Coleoptera follow after the "order," which includes our marvellous "six-legged rivals." In the Introduction, the stability of inorganic substances and the instability of organic matter are, in our opinion, somewhat too strongly insisted on. Dead organic matter, if preserved from the attacks of certain minute living beings, is far less destructible than might at first sight appear. The comparison of the living body to a machine is not free from dangers, and may easily mislead the student.

Still, notwithstanding these points and certain others on which issue might undoubtedly be joined, the work is one which, supplemented by judicious oral instruction, will be of great value to junior students. The fact of its appearance is, we think, a hopeful indication that the Natural Sciences are becoming more widely recognised as an essential part of a sound education, and are being taught upon sounder principles. From the abridgments of Goldsmith and the distortions of Buffon, which "*consule Planco*" were put in the heads of boys who showed a taste for Natural History, to a manual like the present is indeed a satisfactory change.

Pyrology, or Fire-Chemistry. By W. A. Ross. London: E. and F. N. Spon.

It may, perhaps, be questioned whether chemical reactions produced in the dry way, and at temperatures ranging up to full redness, have been as thoroughly studied, whether for systematic or for analytical purposes, as the changes and decompositions occurring in the wet way at temperatures not ordinarily exceeding the boiling-point of water. We have, to be sure, pyro-chemical operations on the large scale in abundance. But have all the processes of the metallurgist, limited as they are in their objects, and in the number of bodies operated upon, been found capable of scientific explanation in accordance with received theories? We have, on the other hand, in blowpipe analysis, a body of methods by which the presence of most inorganic bodies may be detected with no less ease than precision. But without at all undervaluing the results of such men as Berzelius, Plattner, Forbes, and others, we may still ask whether the standard treatises on the use of the blowpipe include every operation by which the presence of elements or of their compounds "can be discovered in the dry way?" This question Major Ross answers in the negative. Some time ago he communicated certain interesting papers to the "Chemical News," in which he made known a number of novelties which promise, at least, to be useful. Whether these new methods have been tested by mineralogists and chemists, and if so with what result, we are unable to say. We cannot lay our hands upon any memoir, English or foreign, in which they are criticised. Among these novelties, real or imaginary, are "the vesiculation of borax with oxides dissolved in it, and the corresponding crystals, which form on the surface of the vesicle, laid on cotton in ordinarily moist atmosphere; the vesiculation of boric acid containing alkaline traces, and the detection of potash in them by breathing on the vesicle; the violet colour given by cobalt oxide to phosphoric acid, and the means of thus quantitatively estimating alkalies, which turn blue in certain proportions; spherospheres, or contained balls, formed by cobalt oxide in boric acid beads; metallic-looking films formed over beads of boric and phosphoric acid held in a good hydrocarbonous pyrocone; decolouration of cobalt with soda by arsenic acid; delicate reactions of oxide of silver in phosphoric acid, by which it can be detected in most galenas; structure of pyrocones; cobalt solution, reaction given by lime; reactions of chlorine and fluorine; curious reaction of soda in pyrophosphate of lime; separation of substances, especially of metals in alloys, by utilising their different attractions for heat; the use of aluminium plate as a support; quantitative assay of sulphide of copper by oxide of lead in phosphoric acid; separation of silica, alumina, ceria, and the alkaline earths, including didymia and lanthana, by means of their behaviour in boric acid; quantitative determin-

ation of chemical water in clear fused boric acid, by means of a magnesian borate ball; separation of silica and alumina by lime borate balls; separation of didymium and lanthanum borates from ceric oxide; detection of fluorine, chlorine, and sulphur by means of oxide of copper in a phosphoric acid bead; detection of phosphoric acid in tourmaline by boric acid; new reaction obtained by a solution of manganese in sulphuric acid; mangano-cobalt solution with the same view; artificial zeolite formed by heating a mixture of potash and pure alumina, for the purpose of detecting alumina or lime, or caustic alkali; yellow and brown oxides of thallium obtained on aluminium plate from the metal; detection of sulphuric acid by the effervescence caused by adding a drop of water to a natural sulphate (as gypsum) which has been fused with soda on aluminium plate; decrepitation observed to be peculiar to crystalline forms; solution and separation of silica by boiling with boric or phosphoric acid dissolved in water; sublimation of gold, silver, and other metals by fusing them in the oxidising flame with a minute proportion of lead or charcoal over aluminium; the curious crystallisation of soda combined with a small proportion of lime; the determination of the mineral constituents of animal or vegetal organisms by burning the latter on a bead of boric acid." It is utterly out of our power to judge these novelties in the only fair manner; that is, by working through them one by one and deciding in how far the author's observations and methods can be actually verified. But this is a task which ought not to be neglected, and we are strongly of opinion that, whatever might be the result, such an undertaking would be well worth the while of any student endowed with the needful leisure, skill, and patience. Even if we suppose, for argument sake, which is extremely improbable, that the author should he found mistaken in every instance, the detection of his errors could not fail to be profoundly instructive.

We have no great respect for writers who take up some subject in an unsystematic way, and after a desultory course of reading think themselves entitled to lay down the law and to point out the supposed mistakes of received authorities. Of such men, and of their productions, every page of which testifies to the want of all fundamental intellectual discipline, every scientific critic is absolutely sick. They swarm upon us like a new plague of flies. But Major Ross belongs to a totally different category. He is no "paper-philosopher," but an earnest, careful, persevering worker, possessed of a fruitful and suggestive mind; and his conclusions, therefore, however unexpected, cannot be without value. We strongly recommend his work to the attention of chemists and mineralogists in the belief that they will find it both interesting and useful.

Modern Naval Hygiene. By Dr. LEROY DE MERICOURT, Chief of the Statistical Department of the French Navy. Translated from the French, by J. BUCKLEY, Staff-Surgeon, H.M.S. *Endymion.* London and Portsmouth: Griffin and Co.

THIS little work is, as the translator remarks in his Preface, "the first attempt to present in an English dress the mass of knowledge we have acquired" on the means of preserving the crew of a vessel in the best possible state of health. The subject, as a moment's reflection will show, is of grave interest—not merely to naval surgeons and to the authorities of Her Majesty's fleet, but to the whole nation. A sickly crew means simply an inefficient ship. The author enters upon his task by calling attention to the great difference which exists in the health of the sailor according as he is engaged,—on the one hand, in the fisheries and the coasting trade, or, on the other, in long voyages, whether in the Navy or in the Merchant Service. In the latter case he is exposed, for long consecutive periods of time, to two sources of danger—over-crowding and the effluvia from the bilges. At one time there was a third, and yet more pressing evil—sea-scurvy, arising chiefly from the difficulty of obtaining a due supply of fresh provisions. This difficulty having been mainly overcome, there remains the great question of ventilation—the supplying, constantly and regularly, pure air in every part of the ship, and the removal of all offensive emanations and of their sources. This question, not always successfully solved on land, is far more difficult at sea. It is obvious that the ventilating arrangements which might prove perfectly adequate during a cruise in the Channel may be found very deficient on a voyage down the Red Sea. The matter has been further complicated by the recent changes in naval architecture, and especially by the introduction of steam-power in men-of-war. The low free-board, the smaller number of the ports, and their lower position, are not in favour of ventilation. The space formerly allotted to the men is curtailed by the engines and coal-bunkers, over-crowding is increased, and the atmosphere of the depths of the ship is more vitiated. On the other hand, the increased rapidity of the voyages and the more frequent calling in harbour counteract to some extent the evils of over-crowding. In the most modern ironclads the number of men is reduced, and the sleeping space is nearly doubled. But we have to take into account not merely the number of cubic feet of air per man, but the ease and speed with which it can be changed in all weathers. In screw-steamers the temperature is often excessive. "In the gun-boat *Eclair*, during the summer of 1855, at Algiers, when the thermometer in the shade stood at 95°, 158° and even 167° F. were registered in the stoke-hole!" Such a temperature is not only directly injurious to the men, but greatly promotes decomposition in all organic bodies. "The process of blowing

out the boilers floods the bilges with hot water loaded with saline and greasy matter." One of the evils of the high temperature is the thirst of the men, and the immoderate quantity of water—plain or acidulated with vinegar or lime-juice—consumed. "In Europe it amounts to 3 or 4 quarts, and is at least doubled in the tropics." We have observed in various chemical works that the men employed in very hot situations quench their thirst with water to which a trace of sulphuric acid has been added: according to our observation and personal experience this is a far safer beverage than any dilution of the vegetable acids, especially vinegar.

A section of this work is devoted to an exposure of the evils arising on board French ships from the use of cooking utensils lined with an alloy of tin and lead. As a matter of course lead-poisoning manifests itself in all its insidious forms. French cooks, by sea and land, appear to have an unfortunate predilection for this deadly alloy, which they call *la claire*. No metal ought to touch human food save iron or silver. The former has been too often rejected from the unfortunate mistake of taking colour into account—an idea which belongs to the dye-house, and should be totally unknown in the kitchen.

In conclusion, we feel bound to express our opinion of the high value of this book, and we hope that it will meet with a wide circulation among all persons interested in nautical matters.

Rambles in Search of Shells, Land and Fresh-water. By J. E. Harting, F.L.S., F.Z.S. London: J. Van Voorst.

It may be asked why our land and fresh-water shells have hitherto enjoyed so little popular attention? They are by no means deficient in interest and beauty; they are more easily captured than insects, and their preservation is easy. The subject, as the author remarks, may easily be studied in connection with botany, or, we may add, with entomology. Why, then, should land shells be so much less regarded than their oceanic kinsmen whose habits can be less easily studied? The entire number of species of terrestrial and fresh-water Mollusca found in the British Islands does not, as far as at present known, exceed 120. By the way, whilst we think too great attention cannot be paid to recording the geographical distribution of every form of organic life, we have never been able to understand the mania for "British specimens" common among a certain class of naturalists, or, we might rather say, of collectors. England is no natural zoological province, but merely a portion of Western Europe which has become detached from the mainland by the gradual action of the sea. The naturalist who excludes all French or Belgian specimens from his cabinet

should—by a parity of reasoning—refuse to admit all that have been captured in Ireland or the Channel Islands.

We are glad to find that the author speaks somewhat contemptuously of the conchology of the past. We have sometimes, in quiet nooks where ancient ideas still linger, met with extensive collections of shells, duly labelled and arranged, and carefully preserved from dust and damage. But the collector did not in the least trouble himself with the nature, structure, and functions of the creatures who somewhile tenanted his shining specimens. To understand them was no part of his object. Such a pursuit is not a Science, but merely a "fancy," and may be placed upon the same level as collecting postage-stamps. But, as Mr. Harting well remarks, "the conchologist has given place to the malacologist, who, not content with examining, describing, and naming the shell independently of its inhabitant, curiously questions the latter as to its internal structure."

The author's object has been to give clear and correct information concerning our indigenous land and fresh-water shells, without being alarmingly technical or systematic. He gives a succinct account of the internal structure of the Mollusca, and notices their peculiarities of respiration, locomotion, and reproduction. Their classification is briefly treated, and the reader is then asked to accompany the author, in spirit, in shell-hunting rambles "over the London clay, over the chalk down, and through the moist beech-woods of Sussex,"—a method of imparting instruction which may perhaps be pronounced unsystematic, but is certainly more attractive than a description of genera and species in their due order. The illustrations have been "carefully drawn and coloured from recent specimens," and not, according to a too common practice, copied from other books. We think this little manual well calculated to attract votaries to this somewhat neglected branch of natural history.

The Dawn of Life; being the History of the Oldest-Known Fossil Remains. By J. W. Dawson, F.R.S., &c. London: Hodder and Stoughton.

We have here a monograph of the *Eozoon Canadense*, the most ancient relic of the animal world hitherto discovered, or, as the author more magniloquently expresses it, "the earliest known representative on our planet of those wondrous powers of animal life which culminate and unite themselves with the spirit-world in man himself." The subject is of profound interest to the geologist and biologist, involving as it does "the opening of a new era in geological science," and pushing the first origin of animal life backwards in time for untold ages. But that it

throws any light on the question of Evolution, favourable or unfavourable, we do not see.

The author proposes "to give a popular, yet as far as possible accurate, account of all that is known of the dawn-animal of the Laurentian rocks of Canada," including a description of the formation itself; "a history of the steps which led to the discovery and proper interpretation of this ancient fossil; the description of *Eozoon*, and the explanation of the manner in which its remains have been preserved; inquiries as to forms of animal life, its contemporaries and immediate successors, or allied to it by zoological affinity; the objections which have been urged against its organic nature, and the summing up of the lessons in science which it is fitted to teach." The latter section of the work is undoubtedly its feeblest part, the earlier chapters being a clear record of facts.

The Laurentian rocks were first recognised as a special geological formation in Canada, where they are strongly developed in a range of hills to the north of the St. Lawrence valley, and named the Laurentides by the old French geographers. They are "the deepest and oldest of all the formations known to the geologist, and more thoroughly altered or metamorphosed by heat and heated moisture than any others." They formerly received the name of Azoic, being, as was then considered, utterly destitute of all traces of animal or vegetable life, but are now designated Eozoic, as "those in which the first bright streaks of the dawn of life made their appearance." The same formation appears in the Adirondack mountains of the State of New York, and in various patches along the American coast from Newfoundland to Maryland. "The older gneisses of Norway, Sweden, and the Hebrides, of Bavaria and Bohemia, belong to the same age, and it is not unlikely that similar rocks in many other parts of the old continent will be found to be of as great antiquity."

The recognition of the Laurentian formation was not by any means at once followed by the discovery of its characteristic fossil, the *Eozoon*. The extensive alteration which the rocks had undergone rendered such a discovery little probable. Lyell, Dana, and Sterry Hunt, however, inferred that there were certain sound reasons for believing that organic remains might be detected even here. They argued substantially thus:—If the Laurentian rocks are altered sediments it follows, from their vast extent, that they are an oceanic deposit. But had there been no living thing in the water, they would merely have consisted of the sandy and muddy *débris* abraded from igneous rocks by the action of the sea. But the Laurentian formation contains beds of limestone above a thousand feet in thickness, and extending for hundreds of miles. Now, limestone is an organic formation. "When," as the author remarks, "we find great and conformable beds of limestone, such as those described by Sir W. Logan in

the Laurentian of Canada, we naturally imagine a quiet sea-bottom in which multitudes of animals of humble organisation were accumulating limestone in their hard parts, and depositing this in gradually increasing thickness from age to age. Graphite is another important constituent of the Laurentian rocks, occurring not in veins or fissures, but in regular layers in the substance of the limestone or gneiss, and forming, according to the author's calculation, in one division of the Lower Laurentian of the Ottawa district, an aggregate thickness of 20 to 30 feet. But vegetable life is the only known agency capable of withdrawing carbon from the carbonic acid of the atmosphere, and depositing it as a constituent of rocks. Hence the existence of plants in the Laurentian age, as well as of animals, becomes highly probable. The Laurentian formation, further, exhibits beds of iron oxide, sometimes 70 feet in thickness—another evidence of organic action.

The actual discovery of organic remains was made by Sir W. Logan, and announced at the Springfield meeting of the American Association for the Advancement of Science in 1859. The organic nature of the *Eozoon* was not, however, at once admitted. Certain men of science maintained that the new-found fossil was of inorganic nature. A discussion ensued, in which Dr. Hunt, Dr. Carpenter, and others took part. Slices of the specimens, in comparison with similar sections of every variety of Laurentian, primordial, and Silurian limestones, and of serpentine marbles, were microscopically examined with ordinary and polarised light. Dr. Hunt undertook a chemical investigation of the associated minerals, the final conclusion being that the structure was organic and foraminiferal, and that it could be distinguished from any merely mineral or crystalline forms occurring in these or other limestones.

The fossil, then, is "the skeleton of a creature belonging to that simple and humbly organised group of animals which are known by the name of Protozoa." It has kindred still existing, and it is even possible that a living specimen of the *Eozoon Canadense* itself may be dredged up from the waters of the Atlantic or Pacific. Foraminiferal animals have, as a whole, been diminishing in size in the lapse of geological time. On this subject the author remarks—"It is, indeed, a fact of so frequent occurrence that it may almost be regarded as a law of the introduction of new forms of life, that they assume in their early history gigantic dimensions, and are afterwards continued by less magnificent species."

The last chapter of the work—"The Dawn-Animal as a Teacher in Science"—is of a much more speculative and less trustworthy character than what precedes. The *Eozoon's* teachings are too evidently "inspired" by Dr. Dawson. The author places great confidence in the silence of the "stone-book." "We have," he says, "for example, no connecting-link between

Eozoon and any form of vegetable life." Be it so; the fact proves absolutely nothing. The further back we go in our researches the greater number of forms—both of vegetable and animal life—must have disappeared, and left not a trace behind. Why should the connecting-links have been especially preserved? And if they had, would not those upon whom the mantle of Agassiz has fallen, and who talk of "prophetic analogues," refuse to recognise them? What can we think of the following passage?—"There may perhaps be higher intelligences that find it equally difficult to realise how life and reason can manifest themselves in such poor houses of clay as those we inhabit? Dr. Dawson cannot imagine that "clay" is a constituent of the human system. Why, then, does he use language which may mislead men of defective education, and which is certainly more worthy of a penny tract or of a "goodie" story-book than of a scientific treatise? Had he concluded his book with the seventh chapter he would have done a better service to science and to the public.

Fifty-fifth Annual Report of the Board of Public Education of the First School District of Pennsylvania, comprising the City of Philadelphia, for the Year ending December 31st, 1873. Philadelphia, 1874.

This volume gives a detailed account of the public schools of all grades in the city of Philadelphia, including the numbers of the pupils, the names of the teachers, the subjects taught, &c. We notice as a curious fact that in schools for boys the teachers, as a rule, appear to be females. What has been the motive which led to so singular an arrangement, or what are the benefits—real or supposed—resulting, we are not informed.

The President's Report is an ably composed document, and must be interesting on this side the Atlantic as showing the projects and the hopes of many thoughtful men in America. We are struck, in particular, with this passage:—"Let me ask you, gentlemen, does not justice demand that we should place within the reach of girls the same unrestricted privileges for pursuing the higher branches of learning that we extend to boys?" It might as well be asked—"Does not justice demand that we should place within the reach of girls unrestricted facilities for acquiring a knowledge of military drill, seamanship, or the use of heavy artillery? We fail to see that science or the human race are likely to be benefitted by enticing or thrusting normal women into pursuits which—not by any arbitrary convention, but by a process of natural selection—have been hitherto reserved for the male sex. We fear that Institutions where girls may "seek graduating honours" will merely

tend to develop a class of epicenes possessing the defects of both sexes and the merits of neither. We submit that mannish women should be regarded with the same abhorrence which we instinctively mete out to womanish men.

The writer, in a subsequent passage of his report, discusses the question of compulsory education, which in America, as in England, appears to be attracting great attention. It appears that in the Philadelphia district, although 100,000 pupils attend the public schools, yet no fewer than 20,000 children are stated to be " growing up in ignorance and vice." This is a much larger proportion than we should have expected. " To give pauperism its death-blow we must rescue the children now beyond the pale of our public schools." But will education, even if universal, really extirpate pauperism ? We fear not. We must remember that, like most other possessions, the value of a good education is governed by the laws of supply and demand. The more general it becomes, the less is its value to the individual. Where educated men are few they can name their own terms, and, except dishonest or dissipated, may make sure of a comfortable position. But where there are more educated men than spheres of activity requiring their services, some must certainly go to the wall. Few persons in modern England are in a more deplorable position than such as have received a good education, but have no special knowledge of any profession, business, or manufacture. Compulsory education, by increasing the number of this class, will merely make the competition among them more frantic. It is—at least as far as England is concerned—an error to suppose " illiteracy " the only, or even the main, cause of pauperism. We see men scarcely able to sign their own names, or to speak their native language with moderate accuracy, amassing millions. We see, on the other hand, men of wide and profound culture, aad even of original thought, earning a bare pittance. We can scarcely point out an instance where a discoverer, an inventor, a man who has enlarged the boundaries of human knowledge, has accumulated a fortune by his labours.

The pauper, again, often inherits a low vital tone : no school can cure that. He frequently inherits an intense craving for alcoholic excitement. Can this be overcome by any amount of literary culture ?

Statistics, the writer holds, " prove by an inexorable logic that ignorance is the most prolific source of crime." They prove certainly that criminals are generally ignorant ; but these two propositions, if carefully examined, will be found not identical. The man of the criminal type is ignorant because he has no innate love for knowledge ; but if you force knowledge upon him it may happen that you merely make him a more formidable enemy to society. The fallacy in question is something like that into which George III. fell in his inquiry into the cause of longevity : he found that all the very old men who came under

his observation agreed in one point, and in one only—they were all given to early rising. Hence the king inferred that early rising was the cause of their longevity, and that any one by persistently rising early might prolong his days; but the fact is that only men of exceptionally vigorous constitutions can regularly, and of their own free will, rise early. Such men attain a great age in virtue of their unusual vigour, of which the tendency to early rising is not the cause, but merely a sign. A feeble person, by persistently rising early, is more likely to shorten than to prolong his days.

To return: it must not be thought that we are in the least favourable to popular ignorance; but we cannot share all the dreams in which some of the more sanguine friends of education indulge. Universal literary culture will suppress pauperism and crime in the very same year when free trade effects the final abolition of war. Till then both "world-betterers" and lookers-on must possess their souls in patience.

Reports on the Physical, Descriptive, and Economic Geology of British Guiana. By C. B. Brown, F,G.S., and J. G. Sawkins, F.G.S. London: Printed for Her Majesty's Stationery Office, and sold by Longmans, Green, and Co., and E. Stanford.

A full and accurate knowledge of the resources and capabilities of every part of the British Empire must be of the utmost value to our statesmen, our Government officials (whether military, naval, or civil), and not less to our merchants and manufacturers. We are therefore glad that such tasks as the geological survey of the rich and beautiful province of British Guiana have been undertaken. We may indeed wish that these investigations were pushed forward with greater zeal, and that the expeditions sent out had the opportunity to enter more minutely into the mineralogy and palæontology of the region. Perhaps in time our wishes may be gratified. The mineral wealth of the country does not seem great. There is an inexhaustible supply of a pure white sand, especially in Demerara, admirably adapted for the glass manufacture. Clays are found of various qualities,—some suitable for hydraulic cements, and others for porcelain. Sandstones suitable for paving and building abound in the interior of the colony, but the expense of transit to the coast is at present too great to admit of its being utilised. Iron and manganese are met with in various districts. Gold has been very extensively sought for, and actually found, though not in remunerative quantities. The workings of the British Guiana Gold Mining Company, up the Cuyuni River, have been abandoned. The greatest wealth of the colony consists in its rich agricultural

soils, some of which contain as much as 0·846 of nitrogen, whilst phosphoric acid reaches 0·185, and potash 0·345 per cent. The authors give a catalogue of rocks and minerals collected for the Geological Museum, in Jermyn Street. That institution, we may here remark, will have to undergo a great transformation if ever it is to represent the economic geology of the British Empire. The amount of space at its disposal is ridiculously inadequate.

The authors do not appear to have been well supplied with resources for a thorough examination of the country. Repeatedly we find in their reports passages similar to the following:—"We regret that our means of supporting labourers with the necessaries of life prevented us from carrying our examinations to the extent we would recommend." We must confess that we like to see scientific work of any kind done thoroughly. To send out qualified men, and to support them so ill that they are compelled to leave their task half accomplished and to hurry over matters which require careful investigation, is a very mistaken economy.

The surveys of the territories undertaken by the Federal Government of the United States include an account of the vegetable and animal productions of every district, as well as of its rocks, minerals, and soils. Nothing of the kind has been undertaken in the survey of British Guiana, a land literally teeming with interesting productions. For this omission we are far from blaming Messrs. Brown and Sawkins. They have evidently made good use of their time and limited facilities, and deserve great credit for the manner in which they have fulfilled their task. But we regret that a more numerous and better appointed expedition was not sent, whether by the Imperial Government or the Provincial Legislature.

The work is illustrated with a large and valuable map of British Guiana, and with several sections and diagrams.

The Annual Address of the Victoria Institute or Philosophical Society of Great Britain, June 7, 1875. By Rev. R. MAIN, F.R.S., "Radcliffe Observer." To which is added the Report for the Year. London: R. Hardwicke.

A COMPLETE review of this "address" may be pronounced difficult or rather impossible in any journal which does not profess to deal with theological questions. We must, therefore, confine ourselves to the notice of certain incidental passages.

As a matter of course, we find a reference to the works of Mr. Darwin, which are brought forward in illustration of the assumed tendency of the present day to accept "a clever hypothesis, supported on some exhibition of facts," without the neces-

sary thorough-going scrutiny. Mr. Main, it seems, rejects the doctrine of Evolution because Professor Nicholson's conclusions, which *seem* to have been formed from a very careful consideration of the subject in *some* of its branches, *seem* to show that Darwin's theories are of very limited application." The words we have italicised throw some light on the logical rigour which Mr. Main would substitute for the rash theories and the love for hasty and paradoxical generalisation of which he accuses the nineteenth century. "The student of natural philosophy," he continues, "is, in my opinion, quite justified, on philosophical grounds, in declining to accept the ancestry offered him." Now, unless Mr. Main is a biologist, his opinion on such a question is not worth recording. He is exactly like a lawyer who should pronounce an opinion on the bearing of certain documents without understanding the very language in which they were drawn up!

After an excuse for not entering further into the Darwinian controversy, the author gives a brief survey of "the most inportant physical discoveries, chiefly astronomical, which have been made during the last few years, being careful to avoid details, and to consider them *only with reference to their bearing on religion*." In this survey it is interesting to note that he admits substantially the nebular theory of Laplace, which but a few years ago was proclaimed essentially atheistic in its tendencies, and was denounced accordingly with no less acrimony than is now bestowed upon the doctrine of Evolution, of which, indeed, it forms an integral portion. Can Mr. Main and his friends learn no lesson from all this?

The author then makes "a passing allusion to two books recently published, which exhibit perhaps the lowest stage of religious belief which has been given in this century as the result of the final and sober conclusions of two very deep thinkers, devoted the one to the study of philosophy and the other to that of biblical criticism." The works in question, Mills's "Essays on Religion" and Strauss's "Old and New Faith," do not come within our cognisance, and we must therefore pass over the examination which they receive from Mr. Main. The remainder of the pamphlet is taken up with a subject which is by this time almost thread-bare—the "Belfast Address." We have already examined this address and have declared that, in our humble opinion, Professor Tyndall went beyond the legitimate sphere of science and made a too great use of his imagination. There is consequently the less need that we should revive the discussion. One passage, however, must claim a brief notice. Says Mr. Main—"Why is Giordano Bruno set so prominently before us but because he revived the doctrine of atoms, though in a very confused way, and asserted pantheistic principles; and because he was a martyr of science, and thus a rare (?) opportunity was given of showing the cruelty and obstructiveness of the Church?" Does the author mean to deny the "obstructiveness of the

Church" or to doubt that from the days of Pericles down to the present year Ecclesiasticism has persecuted science? If so, he is a man with whom no discussion can be held. Discoverers are not now, indeed, rewarded with the dungeon or the stake; but let any one merely read over the denunciations of the Royal Society, the British Association, and the late Society for the Diffusion of Useful Knowledge uttered by ecclesiastics from the time of Charles II. to our own times, and he cannot fail to see that though the power to make "martyrs to science" is wanting, the desire has not slackened. We can point to many attacks upon science made, ostensibly at least, in the interests of religion, and far more unwarrantable than anything in Dr. Tyndall's speech. Nay, it is far from improbable that this speech was merely an injudicious counter-raid. We would refer here to the custom of preaching special sermons to, about, or perhaps rather *at*, the British Association, a custom which we think has been adopted in every town where that body has held a meeting. Too many of these sermons intimate that science is a very dangerous and questionable thing and requires very closely watching. Now, being desirous to prevent these unnecessary and unseemly conflicts between religion and science, we would suggest that all Tyndall-discourses on the one hand, and all these sermons on the other, shall be dropped. Let the clergy of any town where the British Association may meet preach as they would at any other time of the year. Let all scientific theories be suffered to stand or fall on their own evidence, without being considered amenable to any theological tribunal, and let religious dogmas receive a corresponding immunity from scientific jurisdiction. Or, putting the matter in another light, let theologians and anti-theologians, if fight they must, no longer select the territories of science for their battle-field. We fear, however, that such a proposal would be very unfavourably received by Mr. Main and his friends of the Victoria Institute.

Annual Report of the Board of Regents of the Smithsonian Institution. Washington: Government Printing Office. 1874.

This volume is particularly valuable as containing elaborate biographical notices of two illustrious men of science not long ago deceased—Charles Babbage and Louis Agassiz. The former, we fear, in his own country at least, scarcely occupies that high place in public estimation to which he is so fully entitled. His enmity to street music is a standing joke among those who never think intensely, and who therefore cannot comprehend the distracting effect of such noise upon a busy brain. But we almost question whether in this matter Babbage did not "strain at a gnat and swallow a camel." Bad as may be the grinding-organ and the German band, they must yield the palm of nuisance

to the monotonous throb of the "mission-room" bell and the solemn howls of the harmonium in the house of a musical neighbour.

Minds of a somewhat higher order remember the name of Babbage in connection with an unfinishcd calculating machine which, in due obedience to the modern gospel of success, they class with the attempts of visionaries to square the circle or find the philosopher's stone. Now, that Babbage was not a successful man we admit. It is perfectly true, quoting from the work before us, that "there was not a place which he ever sought that he gained. He aspired to the Professorship of Mathematics at the East India College at Harleyburgh, to Playfair's chair at Edinburgh, to a seat at the Board of Longitude, to the Mastership of the Mint, and to the office of Registrar-General of Births and Deaths, and failed in all." His *magnum opus*, the great analytical engine, never was completed, But in the opinion of the numerous and eminent mathematicians and engineers who had examined the inventor's plans, success, in the fullest sense of the word, was merely a question of the needful funds. The machine was intended to contain a hundred variables, each consisting of 25 figures; it would calculate a thousand values (of *e.g.*, a, b, c, d by the formula—

$$p = \frac{\sqrt{a+b}}{c \quad a}\Big)$$

print them, and reduce them to zero. "When the machine wanted a tabular number it would ring a bell and then stop itself. On this the attendant would look at a certain part of the machine and find that it wanted the logarithm of a given number, say of 2303. He would then go to the drawer, take the required logarithm card, and place it on the machine. Upon this, the engine would first ascertain whether the assistant had or had not given it the correct logarithm of the number: if so, it would use it, and continue its work. But if the engine found that the attendant had given it a wrong logarithm, it would ring a louder bell, and stop itself. On the attendant again examining the engine, he would observe the words WRONG TABULAR NUMBER, and then discover that he had really given the wrong logarithm, and, of course, would have to replace it by the right one."

That such a conception should remain incomplete even after its possibility was demonstrated, is a disgrace to the age. "There was not an invention connected with his name, and in mathematical mechanics he ranks among the foremost the world ever produced, which, in the opinion of the best disciplined minds of his day, he could not have perfected had sufficient pecuniary means been at his command.

Among his literary productions we find honourable mention made of the "Ninth Bridgewater Treatise." His no less remarkable work, the "Decline of Science in England," has escaped

notice. We fear that not a few recent circumstances could be found to support the position of the author, and that the tide which he regretfully pointed out has not yet turned.

The career of Agassiz is one which we must contemplate with a strange mingling of satisfaction and regret. We rejoice that he effected such great things for science; we sympathise with his enthusiasm in the cause of discovery; we fully appreciate the fearlessness with which he attacked many of the most rooted prejudices, in connection, for instance, with the notion of a broad and abrupt boundary line between man and the rest of the animal creation; but we view with astonishment and grief his attitude in connection with the great doctrine of evolution. He received the first announcement of a theory which has shed such an invaluable light upon organic science, not like a philosopher, critical, or if you will, even sceptical, but like an eager partisan who feels his interests at stake. The explanation of this painful fact may be deduced from the work before us. His brain, like that of many a noble follower of science, was giving way under the pressure of severe and continuous study. Had he lived longer he would have outlived his real self. There can be no doubt that at the time when the views of Darwin were first brought under his notice he was already labouring under incipient cerebral disease. We do not desire to dwell further upon the spectacle of the decay of a genius so exalted. Agassiz was a man for whom the world should feel thankful, and with whose final short-comings it should deal reverently.

Magnetism and Electricity. By FREDERICK GUTHRIE, Professor of Physics at the Royal School of Mines. With 300 illustrations. London and Glasgow: William Collins, Son, and Co. 1876.

WORKS on electricity have somewhat multiplied of late, but with one or two exceptions they have belonged to the popular and slight class of books, rather than to the solid and precise student's manual. For many years De la Rive's "Electricity" was the standard work on the subject, supplemented, of course, by Faraday's admirable "Electrical Researches;" and, from the more popular point of view, books like Noad's "Manual." Now we have Clerk Maxwell's magnificent mathematical treatment of electrical phenomena, and the numerous papers of Sir William Thomson, which form a treatise in themselves, and one or two works like Fleeming Jenkin's "Electricity," in which many of the most recent mathematical deductions are introduced, and are treated in a more or less popular manner. Dr. Guthrie's work belongs to this last class. It is based upon the lectures which he has been in the habit of giving, during the last six years, at the School of Mines to mining students and science

teachers, and he now brings it forward in an extended form in the hope that the work may be of service to a larger class of the community.

The first book, consisting of ten chapters, treats of Frictional Electricity, and commences with an account of attraction and repulsion, and the dependence of the former on induction. Under the head of Electrical Machines, the new machines of Holtz and Bertscl are described and figured, and the clearest account of the former we have ever seen is given here. Condensers and the effect of the discharge follow, a short chapter on Dielectrics, a chapter on various sources of electricity besides friction, and, finally, a chapter on Electrical Measurement.

In this we find some extremely useful definitions, thus as to *unit of electricity*, the author observes:—"Each of two equally charged bodies is said to have a unit of electricity if, when at a distance of one centimetre from one another, the one will repel the other with a force which in one second of time would impart a velocity of one centimetre a second to one grain of matter." Again, as to that difficult definition the Electrical Potential:—"The work which a raised body is capable of doing, if it fell a certain distance, is called the *potential* work of this body, or simply its (mechanical) potential. . . . . Instead of a unit of weight, let us take a unit of electricity. . . . . The electrical potential of a body is the mechanical work which the electricity of the body is capable of doing in passing to the earth, or other indefinitely great reservoirs of electricity of the same kind as the earth's. . . . . Potential may be defined as the preparedness to do work." The chapter concludes with an account of Thomson's Electrometer.

The second book treats of Voltaic Electricity, and consists of twelve chapters, among which may be specially noted those on Measurement, on Resistance Conductivity, Electro-Motive Force and their Measurement, and on the Relation of Electricity to Life. In the latter, we are told, in insectivorous and sensitive plants a current is established at the moment when the leaf is irritated, and when consequently it moves. Again, although there seems to be a somewhat intimate connection between nervous and electrical excitement, the strongest coil-current applied to the head as a cap does not appear to affect the thinking faculties. Neither does the act of thinking appear to produce a current. The third and final book relates to Magnetism, and this is followed by two appendices containing useful experimental hints.

Dr. Guthrie's book, without being a popular treatise in the broadest sense of the term, combines facile expression, and an absence of abstruse treatment, with an account of the more recent mathematical development of the science. It is essentially a student's manual: thorough and precise, without being overloaded with dry details and technicalities. The experimental

details are valuable, as we may be sure that all the experiments described have been repeated at South Kensington, and not merely introduced on the hearsay of others. Altogether the book is to be welcomed as a real addition to our scientific literature.

Catalogue of the Publications of the United States Geological Survey of the Territories. By F. W. HAYDEN. Washington: Government Printing Office.

"THE geologist in charge is desirous of securing, by exchange, the publications of foreign countries on geology, palæontology, and natural history generally, to aid in the formation of a library of reference for the use of the survey of which he has charge.

"He avails himself of this opportunity to again ask those persons or societies that may receive the publications of the survey to reciprocate by sending to him such of their own publications as they may feel disposed, and he believes that he can assure them an ample return, either in books, or specimens, or both.

"The reports of surveys with maps, charts, and sections, transactions of societies, or the publications of individuals engaged in scientific studies, are much desired as works of reference.

"Parties who may look favourably upon the above proposition can send all packages, through the Smithsonian Institution, to the address of Dr. F. W. Hayden, U.S. Geologist, Washington, D.C.

"Societies, libraries, or persons engaged in active scientific investigation, desirous to receive the publications of the survey will confer a favour by communicating their wishes."

Whilst we feel great pleasure in giving publicity to this appeal, we feel it our duty to call attention to the truly magnificent character of the survey now being executed on behalf of the Government of the United States. Not merely the geology, but the mineralogy, botany, zoology, meteorology, ethnology, and antiquities of the whole union are being carefully explored. Will the day never come when a similar survey will be made of the still wider and more varied regions included within the British empire? It would be a glorious contribution to science and a worthy bequest to posterity.

CORRESPONDENCE.

AËRIAL LOCOMOTION.

Sir,—In your issue of October, 1875, you publish a letter from Professor Marey in reply to an article by Professor Coughtrie entitled "Pettigrew *versus* Marey"* which requires a few words of comment on my part. In the article in question Professor Coughtrie, as your readers will remember, prefers what many of them will regard a charge of plagiarism against Professor Marey, and quotes in support a series of parallel passages and dates which one would naturally have expected Professor Marey would either have explained or refuted, but which singularly enough he has not done.

As Professor Marey in his reply makes it appear that I have intentionally or otherwise given a mutilated version of the letter addressed by him to the French Academy, in which he assigns me priority in the discovery of the figure-of-8 and wave movements made by the wing in space, you would do me a favour by publishing that letter in full. In order that the reader may have all the facts before him I will, with your permission, refer to the entire correspondence between the French Academy, Professor Marey, and myself.

(I.) *Letter of Reclamation addressed by Dr. Pettigrew to the French Academy of Sciences.*

"Royal College of Surgeons of Edinburgh,
"March 28, 1870.

"The Perpetual Secretaries of the French Academy.

"Gentlemen,—Having had my attention directed to two papers communicated by Professor Marey to the Academy of Sciences on the 28th of December, 1868, and the 15th of March, 1869,† in which he describes and puts forth as a new discovery the peculiar figure-of-8 movements made by the wing of the insect during its action, it has been suggested to me that in justice to myself I ought to inform the Academy that the figure-of-8 theory of wing movements was first promulgated by me in a lecture delivered at the Royal Institution of Great Britain in March, 1867. An abstract of the lecture was translated into French and appeared in the "Revue des Cours Scientifiques" of 21st September, 1867, along with two other papers—the one by Professor Marey, the other by M. Armand Angliviel. * * * [Here follow references., &c.] I have taken the liberty of submitting a copy of my memoir to the Academy in the hope that it may graciously consider my claim to be regarded as the original discoverer of the *figure-of-8 movements* made by the wings of insects, bats, and birds, when artificially fixed, and of the *spiral and undulatory wave tracks* described by the wings of insects, bats, and birds, when the said insects, bats, and birds are flying at a high horizontal speed.

"I have only to add that my lecture appeared in the "Proceedings of the Royal Institution of Great Britain" under date the 22nd of March, 1867, nearly two years before Professor Marey's first communication to the "Comptes Rendus;" my memoir of which the lecture formed a part having been read to the Linnean Society in less than three months after the lecture was published, viz., on the 6th of June, 1867.—I have the honour to remain, &c."

(II.) *Letter Addressed by Professor Marey to Dr. Pettigrew before Replying to the Letter of Reclamation Lodged by Dr. Pettigrew with the French Academy.*

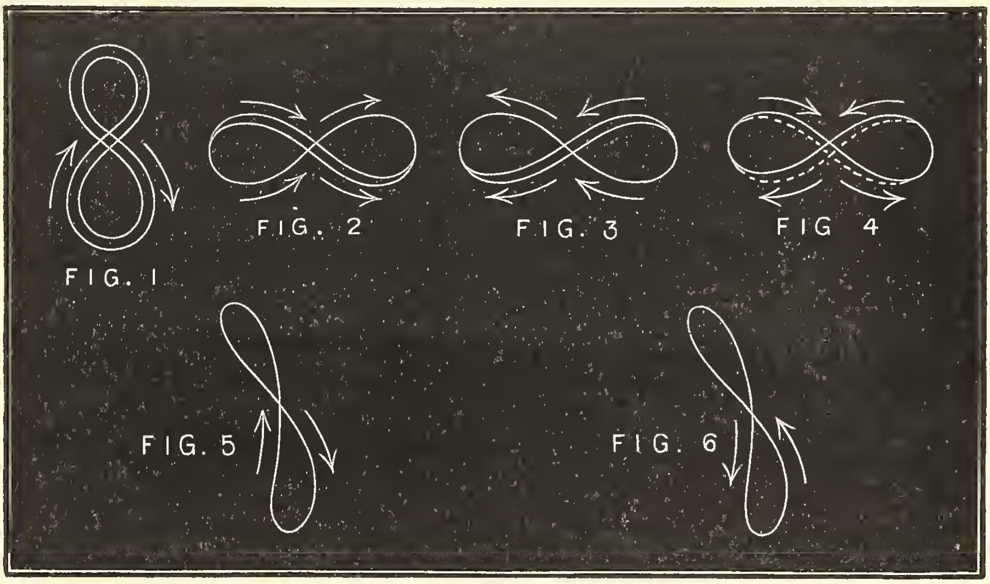
"Paris, April 29, 1870.

"Sir and Honoured Colleague,—I

* Quarterly Journal of Science for April, 1875.

† These communications are printed in the "Comptes Rendus" under the dates specified.

was absent from Paris when your reclamation was produced on the subject of the figure described in space by the wing of insects and birds ; it is on that account that I have delayed to answer your note. It appeared to me that it was best to address myself to you directly. Indeed, I believe there exists between us a pretty considerable disagreement as to the manner in which the figure-of-8 described by the insect is generated. I do not hesitate to acknowledge that in your memoir you have pointed out certainly before me the figure-of-8 presented by the wing of the insect during flight, and I regret not having had earlier knowledge of your work in order to assign to you that priority. One of my audience, however, told me last year that Lacordaire had formerly pointed out indicate. In the copy of my work which I have the honour to address to you you will see at page 171, figure 16, that I find that the two branches of the 8 are engendered by two movements alternating and in opposite directions produced by the wing of the animal, as is shown by the reverse direction of the two arrows. In my experiments I applied gold leaf to a point of the extremity of the wing, and it is the track made by this single point which describes the contour of the 8. In your diagram you ascribe to the two opposite borders the formation of the figure-of-8, so that (diagram 5) the thin border which is formed above at the beginning of the movement passes below at the end of the movement. For me the narrow border does not pass thus from one side to the other



that figure of movement of the wing; but I have not succeeded in finding that indication in his works on entomology. In regard to the interpretation of that appearance of the wing in motion, here is, I believe, the point in which we disagree. On referring to page 233 of your memoir, diagram 5, if I have rightly understood your exposition, the thick line indicates the passage of the thick margin of the wing, the narrow line the passage of its thin border; these two lines are generated in one single movement of the wing, which in the case of diagram 5 is carried from left to right, as the direction of the arrows of the thick border, and when we place a little speck of gold on each of these two borders we see formed two figures of 8 parallel with each other, as I represent it here. [Fig. 1 of present communication.] I consider the changes of plane and all the deviations of the course of the wing as the effect of the resistance of the air, in which the wing is entirely passive. If my imperfect knowledge of the English language has not led me into error, the wing, according to you, would change its form actively, and would rotate on its articulation as if to screw itself in the air. Experiments which I have made by

means of a mechanical insect have shown me that the mechanism of flight may be obtained quite well with wings capable of changing their planes in consequence of the resistance of the air without being able to undergo helicoidal torsion. Finally, as the bird which flies with registering apparatus (Figures 32 and 35) has furnished an elliptical tracing and not an 8, as in the insect, you see, Sir, that the reply to your note to the Academy of Sciences is not easy, and this accounts for my taking the liberty of addressing you to explain the situation and to beg of you to let me know as soon as possible how I can give you satisfaction without entering into a discussion which the reader would have great difficulty in following. May I ask you to send me a copy of your learned Memoir in order that I may study it more at leisure, and profit in the future by your labours in a subject so interesting.—Receive, Sir, &c."

NOTE. In this letter it will be perceived Professor Marey asks me to limit my claim to certain points in order to avoid discussion. Having, however, limited or restricted my claim, Professor Marey insinuates in his reply to Professor Coughtrie that I am not entitled to claim priority in the figure-of-8 and wave movements without at the same time stating that he differs from me as to the manner in which these movements are produced. In other words, he wishes it to be understood that the original discovery of the figure-of-8 and wave movements are of little account in comparison with his explanation of the manner of their production. In reality, however, as Professor Coughtrie showed in his article, and as I pointed out in a letter addressed to Professor Marey in 1870, there is little real difference between us, and Professor Marey deceives himself and his readers when he states that his theory is wholly opposed to mine. The figure-of-8 as originally propounded by me includes, as I endeavoured to show in my letter of 1870, not only all the essential features of Professor Marey's hypothesis, but even its details.

I have described and represented the figure-of-8 made by the margins of the wing (figs. 2 and 3 of present communication); Professor Marey has described and represented the figure-of-8 made by the tip of the wing (fig. 5 of present communication). If, however, the margins of the wing make figures-of-8 during extension and flexion and during the down and up strokes, it is obvious that the tip of the wing, or any point between the margins, will also make a figure-of-8; further, that the arrows indicating these movements will point in opposite directions. There is no escaping from this conclusion. However incredible it may appear, Professor Marey in his letter to the French Academy in reply to my reclamation completely ignored these explanations, touched lightly upon the numerous points of resemblance, and emphasised the minor and supposed points of difference. He has wittingly or unwittingly persisted in making one half of my figure of 8 represent the whole, and has erroneously stated that in my figure of 8 the arrows all point in one direction; whereas in the completed figure they point in opposite directions as indicated at fig. 4 of present communication. My figure of 8 when completed, and it must be completed according to the text, is identical with that subsequently given by Professor Marey. I feel aggrieved at this treatment, as Professor Marey must be well aware that the part cannot in any case stand for the whole. With a perseverance and perversity worthy of a better cause Professor Marey even goes the length of stating that he has accurately counterdrawn what he designates my suppressed figure in the English edition of his "Animal Mechanism" (page 201, figure 86). It is quite true he has drawn and inverted my half figure, but, as Professor Coughtrie points out, he has in so doing totally misrepresented my views. One of two things is quite obvious, either Professor Marey does not even now understand the *rationale* of the figure of 8 as originally described and delineated by me, or (and I should be sorry to adopt this conclusion) he wilfully distorts and mixes up the details. It may be well to mention at this stage that while Professor Marey is aware that the wing of the insect makes a figure-of-8

track in space, he is even now ignorant of the exact course pursued by the wing. Thus he represents the wing as oscillating in a vertical direction, the down stroke according to him being delivered downwards and backwards, and the up stroke upwards and backwards as in fig. 5 of present communication. In reality, and as I long ago pointed out, the wing of the insect is made to vibrate in a very oblique and nearly horizontal direction, the down stroke being delivered not downwards and backwards but downwards and *forwards;* the up stroke being delivered not upwards and backwards but upwards and *forwards*, so (fig. 6 of present communication). This is a vital point. It is a physical impossibility for the wing to act as Professor Marey states. The arrows in Professor Marey's figure of 8 ought in reality to be reversed. To get a continuous series of figure-of-8 loops, or of forwards curves characteristic of progressive flight, the wing must, as I have all along maintained, *descend and ascend in a forward direction.* The tracings obtained by Professor Marey himself prove this conclusively; nevertheless he has failed to divine their precise meaning. That the true action of the wing is such as I indicate is placed beyond doubt by my own experiments with natural and artificial wings published in the "Transactions" of the Linnean and Royal Societies, and by recent experiments described in the "Ninth Annual Report of the Aëronautical Society of Great Britain."

(IV.) *Letter addressed by Professor Marey to the French Academy of Sciences in reply to a Reclamation lodged by Dr. Pettigrew* (*Comptes Rendus*, May 16, 1870).

"It is with regret that I have so long delayed the reply to the note of Mr. J. B. Pettigrew of date 18th April last. The author of that note reclaimed priority in the description of the figure-of-8 path pursued by the wing of the insect in flight. In support of his reclamation the author quoted several passages of a memoir which he has sent to the Academy. I have perused this memoir, and have ascertained that in point of fact Mr. Pettigrew had seen before me and represented in his memoir the figure-of-8 track made by the wing of the insect; that the optical method to which I had recourse is almost identical with his; but that we differ entirely in regard to the interpretation of the trajectory which we have both seen. Our disagreement refers to the direction of the movement of the wing during its course, the cause of its changes of plane, and the inflexions of its course which I ascribe to the resistance of the air. Finally, generalising his theory of the movements of the wing, the English author assigns to the wing of the bird the same trajectory as to that of the insect, whilst I have shown—in my lectures at the College of France, published last year in 'Revue des Cours Scientifiques'—that the wing of the bird moves according to a kind of ellipse of which the great axis is nearly vertical. In presence of these differences I have thought it right to address myself directly to Mr. Pettigrew, to explain to him the complexity of the dispute, and to ask him how I could reply to his 'Just Reclamation' without entering into a discussion which would unnecessarily complicate the question. It is only to-day that I have received the reply of the physiologist of Edinburgh. He holds to establish only 'that he has described and figured before me the figure-of-8 track made by the wing of the insect, and the spiral and undulatory curve which the wing of the insect, the bird, and the bat describes when these animals fly with a great horizontal velocity.' Whether this remark has been made by other naturalists nobody would venture to affirm, but in any case I hasten to satisfy this legitimate demand, and I leave entirely the priority over myself to Mr. Pettigrew relatively to the question so restricted. I hope to be able soon to submit to the Academy my experiments on the analysis of the movements of the bird during flight, in order that the Academy may judge the value of the processes which I have employed in that investigation."

Note. It appears to me that the real restrictive clause is that quoted by me, and not by Prof. Marey; the clause alluded to having reference to the figure-of-8, spiral, and undulatory

movements originally discovered by me, rather than to Prof. Marey's interpretation of the manner in which those movements are produced. This follows because Prof. Marey, before replying to the Academy, and as the reader is now aware, requested me to limit or define the extent of my claim upon the movements in question: the restrictive clause certainly occurs in this connection. Prof. Marey, not content with mixing up two things essentially distinct, states that in my last work ("Animal Locomotion," 1873) I devote ten pages to impugning his theory of the flight of birds, and a special chapter to show to what extent my theory differs from his. These pages and that chapter, I may remark, were first published in 1870, and deal not with the figure-of-8 theory in its integrity, but with certain details which Prof. Marey has since modified. When I wrote them Prof. Marey (*vide* his earlier writings, "Revue des Cours Scientifiques de la France et de l'Etranger," March, 1869) maintained that the wing made *a backward angle of* 45° *with the horizon during its descent, and a forward angle of* 45° *during its ascent.* This view, shown on that occasion (1870) to be untenable, has been greatly modified in Prof. Marey's later works. Those of your readers who compare Prof. Marey's works with my own, and who have perused Prof. Coughtrie's article and examined the parallel passages and dates given in your Journal for April, 1875, will, I believe, have difficulty in accepting Prof. Marey's statement to the effect that "two authors could not easily have treated the same subject with more widely dissimilar methods, and arrived at more different conclusions." Prof. Marey makes light of the parallel passages and dates. He says Prof. Coughtrie "compares texts, and every time he meets with similar expressions in the English and French books he raises a cry of plagiarism, as if it were possible to treat of the flight of birds without speaking of wing, child's kite, inclined plane, sculling, &c." The question, however, has its serious aspect. The plagiarisms, if such they are to be designated, are not—as Prof. Marey would have your readers believe—confined to isolated words; they extend, according to Prof. Coughtrie, to clauses and passages, and these clauses and passages occupy many pages. It is easy to understand how two authors can write on the same subject, at the same time, and express the same ideas *in different language:* it is more difficult to comprehend how two authors can write on the same subject, at an interval of nearly two years, and express the same views in what many will regard as *virtually the same language.*

J. Bell Pettigrew, M.D., F.R.S.,
Professor of Medicine and Anatomy,
University of St. Andrews.

PROGRESS IN SCIENCE.

Microscopy.—M. Ranvier communicates the following mode of preparing sections of bone to the "Archives de Physiologie":—A portion of the shaft of a long bone is procured, and immediately on its removal from the body is plunged into water, and allowed to macerate for the space of a year, the water being frequently changed. At the end of that time the bone will be found to have become as white as ivory, and quite free from any adhering tissue. The object of immediately plunging the bone in water is to prevent the infiltration of the canals and substance of the bone with fat. Sections are to be made with a saw, and these sections are ground down on pumice-stone, and finally polished on a harder material. In order to remove the powdered fragments of bone which have been ground off, and fill the canaliculi and lacunæ on the surface, it is sufficient to scrape the section with a scalpel. It is then placed in a warm solution of aniline dye, and allowed to remain there for two hours, and afterwards dried on a water-bath. The section is next rubbed on a hone, moistened with a 2 per cent solution of common salt. It is then washed in this solution, and permanently mounted in a mixture of equal parts of the solution of salt and glycerine.

Great attention has been paid of late to the staining of tissues with aniline and other dyes. Many structural details are no doubt rendered more distinct; but of course here, as in every kind of microscopical observation, care must be taken to avoid errors of interpretation.

It would appear, from enquiries in various journals, that many microscopists experience a difficulty in securing the cells of preparations mounted in glycerine. At the risk of repetition an effectual mode of preventing leakage is given, although, from the long time it has been in use, it might have been supposed that everyone had been acquainted with it. The object having been placed in the cell, and the cover adjusted, proceed to remove as much of the surplus glycerine as possible: if the quantity is large, as is sometimes the case with over-filled cells, most of it can be taken up by suction with a sharply-pointed pipette; the residue should be carefully absorbed with fragments of blotting-paper applied with the forceps; then, with a camel-hair brush and water, the cell and surface of the cover should be gently cleaned: care must be taken not to disturb the cover-glass. The varnish employed is the solution of shellac in wood-naphtha, known at the shops as "Liquid glue" or "Patent knotting varnish," and effectually resists the action of the almost universal solvent glycerine, if properly applied. The cell having been cleansed as much as possible from surplus glycerine, apply the varnish with a small camel-hair pencil, by hand, running it into the angular cavity at the junction of the cell and cover-glass, taking care, however, that only the smallest possible portion of cell and cover is included in this first coat. When dry, carefully wash the slide under a gentle stream of water; a tap partially turned on, or the ordinary wash-bottle, will answer the purpose. The object of this washing is to thoroughly cleanse the cell from adhering glycerine, which could not be removed while the cover was loose: dry the slide carefully by wiping with a cloth, taking care not to touch the cell and cover, which require a very tender drying with blotting-paper gently applied: the next coat of varnish is now to be laid on, and the turn-table can now be used to advantage, as the cover-glass is not likely to be disturbed. Do not at present coat the whole surface of the cell, but leave some part bare for the next varnishing. When dry, wash again as before, as the success of the whole process depends upon the freedom of the surfaces from any trace of glycerine. After this apply several coats of the varnish, taking care to keep each coat very thin. Never attempt to varnish thickly, as the interior seldom or never hardens. When sufficient varnish has accumulated, as shellac varnish becomes brittle after a time, add, for security, three or four coats of gold-size: this is the most tenacious and reliable varnish

known, and will effectually protect the more brittle liquid glue, Gold-size is rapidly acted upon by glycerine, but when protected with a good coating of shellac is perfectly safe. Too much care cannot be taken in washing and cleaning the surfaces to be varnished from the least trace of adhering glycerine: the entire success of the process, as before mentioned, depends upon minute attention to this point, and if carefully carried out success is certain. The writer has objects mounted in glycerine fifteen years ago free from any trace of leakage. Liquid glue is also effectual in varnishing preparations mounted in castor oil: this useful medium has been much neglected, owing to the difficulty of preventing leakage. The cell and cover should be cleaned, as before mentioned, but, instead of water, benzole—carefully applied with a brush—must be used for the removal of the oil remaining after the application of the blotting-paper. The shellac varnish is to be followed by gold-size, as in the case of glycerine mounting.

A new method of measuring the absorption-bands in spectra has been communicated to the Royal Microscopical Society by Mr. H. C. Sorby. The apparatus consists of a crystal of quartz, cut so that the light passes along the line of the principal axis: the thickness used is exactly 1½ inches. This crystal is placed between two Nicol prisms, the upper one capable of rotation for the purpose of adjustment; the lower one fixed to an ivory circle, 2½ inches in diameter, each half of which is divided into ten parts, and these again into five smaller divisions, so that there is no difficulty in reading off to 1-100th of a half revolution. The light passing through this apparatus, when viewed with the spectroscope, divides the spectrum into eight spaces by seven well-defined bands, which in a prism spectrum are apparently at very uniform intervals, but at a much less wave-length interval at the blue than at the red end. When the apparatus is attached to the micro-spectroscope, the pointer of the circle being placed at zero, the upper prism is rotated so that the centre of the second dark band from the red end of the spectrum exactly coincides with the sodium line, or with the solar line D. All the other dark bands are then in perfectly constant and definite positions, depending on the action of quartz on light of various wave-length. On rotating the ivory circle each band gradually passes from the red end towards the blue, until when the circle comes to the next zero point, commencing the next semicircle, the series of bands is exactly the same as at first. The formula for reducing the degrees of the scale to their respective wave-lengths is given, and also a table for an instrument constructed with a crystal of quartz of exactly 1½ inches in thickness, cut and mounted as described. The chief objections to the apparatus appear to be the difficulty of obtaining and cutting a suitable piece of quartz, and also the size of the apparatus, its length being about 3½ inches, which renders the instrument inconvenient for attachment to the ordinary micro-spectroscope, but it has answered well placed below the stage of Mr. Sorby's binocular spectrum microscope.

The Quekett Microscopical Club have issued a Catalogue of the preparations in their cabinet, containing the total number of 2041 slides. The subjects most largely represented are—Hairs of animals, of which there is a collection of 238 specimens, including a very complete series from Indian and other bats; the seeds amount to 217—these and the greater part of the hairs, especially those from India, were the gift of Dr. M. C. Cooke, as also most of the microscopic fungi, of which there are 69 specimens; the ferns are well represented by 171 slides. The objects can be borrowed for examination by members, under certain conditions, offering great facilities to those who wish to compare at their leisure a series of specimens. The whole collection has been formed by the liberal donations of members of the Club, and it is to be hoped that the annual increase may be a large one, which will certainly be the case if mounters of objects will send their duplicate specimens to the cabinet.

An ingenious little instrument, for readily cleaning very thin covering-glass, has been contrived by Mr. W. W. Jones, of the Quekett Microscopical Club. It consists of a small tube of brass or steel, of about an inch in diameter and

the same in height, into which fits loosely a weighted plug. To the lower end of this plug is cemented a piece of chamois leather. Another piece of leather is stretched upon a flat piece of wood or plate-glass, to form a pad, which completes the apparatus. In using, the tube is placed on the pad, and the thin glass, after breathing on, dropped in; the plug is then inserted, and, holding the tube well down on the pad, any amount of rubbing can be used with perfect safety, the weight of the plug giving sufficient pressure.

The subject of double staining of wood and other vegetable tissues has been experimented upon by Dr. George D. Beatty, of Baltimore: the results will be found useful to those interested in preparing tinted sections. The author has discovered that benzole instantly fixes any aniline colour in vegetable tissues, and also renders them as transparent as oil of cloves. The following are the processes adopted:—A section of wood or other vegetable substance, being prepared for dyeing, is put for five or ten minutes in an alcoholic solution of *roseine pure* (magenta), one-eighth or one-quarter grain to the ounce. From this it is removed to a solution of "Nicholson's Soluble Blue Pure," one-half grain to the ounce of alcohol acidulated with one drop of nitric acid. In this it should be kept for thirty or ninety seconds, rarely longer. It should be frequently removed with forceps during this period, and held to the light for examination, so that the moment for final removal and putting into benzole be not missed. After a little practice the eye will accurately determine the time for removal. Before placing the object in benzole it is well to hold it in the forceps for a few seconds, letting the end touch some clean surface, that the dye may drip off, and the object become partially dry. By doing this fewer particles of insoluble dye rise to the surface of the benzole in which the brushing is done to remove foreign matter. The object should then be put into clean benzole. In this it may be examined under the glass. If it is found that it has been kept in the blue too short a time it should be thoroughly dried, and after dipping in alcohol be returned to the dye. If a section of leaf or other soft tissue be under treatment it should be put in turpentine or *oil of juniper*, as they do not cause so much contraction as benzole. When hæmatoxylin is used instead of magenta, it is followed by the blue as just described. As neither of these dyes come out in alcohol or oil of cloves, the section may be kept in the former for a short time before placing in the latter. The hæmatoxylin dye preferred by Dr. Beatty is prepared by triturating in a mortar for about ten minutes two drachms of ground campeachy wood with one ounce of absolute alcohol, setting it aside for twelve hours, well covered, triturating again and filtering. Ten drops of this are added to forty drops of a solution of alum; twenty grains to the ounce of water. After an hour the solution is filtered. Into this the section, previously soaked in alum water, is placed for two or three hours, or until dyed of a moderately dark shade. When dyed of the depth of shade desired, which is determined by dipping it in alnm water, the section is successively washed for a few minutes each, in alum water, pure water, and 50 per cent alcohol. Finally it is put in absolute alcohol until transferred to the blue. Carmine and aniline blue produce marked stainings, but they are rather glaring to the eye under the microscope. An ammoniacal solution of carmine is used, double the strength of Beale's, substituting water for a glycerine. In this a section is kept for several hours. On removal it should be dipped in water, and then put for a few minutes in alcohol acidulated with 2 per cent of nitric acid; then in pure alcohol; then in the half-grain blue solution before spoken of, from which it should be removed to alcohol; then to oil of cloves. Much colour will be lost in the acid alcohol. The acid is to neutralise the ammonia which is inimical to aniline blue. Magenta or hæmatoxylin may be used with green instead of blue aniline. Iodine green is to be preferred, one grain to the ounce of alcohol. Double stainings of leaves in which red is first used have the spiral vessels stained this colour, other parts being purple or blue. Radial and tangential sections of wood have the longitudinal woody fibres red, and other parts purple or blue. This selection of colour is supposed by Dr. Beatty to be due to the fact that spiral vessels and woody fibres take up more red than other parts, and are slower in parting with it. The blue, therefore, seems first to overcome the

red in parts where there is less of it. It will entirely overcome the red if sufficient time be given. If the blue be used before the magenta aniline, the selection of colour is reversed. The sections should be mounted in Canada balsam, softened with benzole, as the presence of the latter may be beneficial in preserving the magenta. Respecting section-cutting and preparing sections for dyeing.—To cut a thick leaf, place a bit of it between two pieces of potato or turnip, and tie with a string. Cuts may be made along the midrib, or across it, including a portion of leaf on either side, or through several veins. Fine shavings of wood may be used, or pieces rubbed down on bones. Sections of leaves may be decoloured for staining by placing for some time in alcohol; but Labarraque's solution of chlorinated soda is to be recommended for twelve or twenty hours after the alcohol. In twelve hours wood is generally bleached; too long treatment will, however, cause it to fall in pieces. After removing from the bleaching solution wash through a period of twelve or eighteen hours in half a dozen waters, the third of which may be acidulated with about ten drops of nitric acid to the ounce, which acid must be washed out. Next put in alcohol, in which sections and also leaves may be kept indefinitely ready for dyeing. One week instead of forty-eight hours is frequently required to effect the decolouration of large leaves in chlorinated soda, even when they are cut into several pieces, which is advisable. Mr. L. R. Peet, of Baltimore, thinks better results are attained by commencing with a weak dye, say from one-twentieth to one-twelfth of a grain, and slowly increasing the strength of the dye at intervals of from one to three bours until the required hue is obtained. This process guards against too deep staining, and gives a finer tone to the leaves under the microscope.

Engineering, Civil and Mechanical.—*Bombay Docks.*—The question of providing suitable wet docks for the Port of Bombay has for many years been under consideration by the Government in India. Perhaps one cause which has contributed, as much as anything else, to delay commencement on the work, has been the fact that two rival schemes have been in existence, one of which had for its object the location of the Docks on the Elphinstone Estate, and the other at Moody Bay. It having, however, now been determined that the advantages of the former site have the pre-eminence over those of the latter, it has been determined to construct the Docks there from the designs of Mr. Thomas Ormiston, the Engineer to the Bombay Port Trust. Contracts for the masonry part of the work have been let to the firm of Messrs. Glover and Co., Contractors, of Bombay. The first stone of the Docks was laid by the Prince of Wales on the occasion of his visit to Bombay. Further particulars of this work will appear in some future number of the "Quarterly Journal of Science."

Liverpool Landing-Stage,—It will be remembered that on the afternoon of the 28th of July, 1873, the great landing-stage at Liverpool was destroyed by fire, an occurrence which caused no little inconvenience to the passengers crossing the Mersey. The old stage consisted of iron pontoons, which supported five large wrought-iron kelsons on box girders, about 20 feet apart, running longitudinally the whole length of the stage. Across these kelsons were placed pine beams the width of the stage, and varying in thickness from 16 inches to 14 inches by 12 inches. Upon these beams was fastened the longitudinal pine deck, or planking, 6 inches by 4 inches, and crossing this again were greenheart sheathing planks, 6 inches by 2 inches. The whole of this was caulked and pitched, to make it independent of the action of the water and the weather. The new stage consists of the pontoons and large wrought-iron girders as before, but, in place of wooden beams, iron beams have been substituted through the entire length of the stage. The pine deck-planking has been replaced with greenheart, and greenheart sheathing, as before, completes the decks. The new wrought-iron deck-beams weigh nearly 1500 tons, and this fact alone will give some idea of the magnitude of the undertaking, which was successfully completed by Messrs. Brassey, who were the contractors for the work.

A paper has recently been read before the American Society of Civil Engineers, on "The Water Front of New York," which furnishes some very interesting particulars of the engineering works undertaken for the accommodation of the trade of that port. The extent of available accommodation for shipping is shown by the fact that the city has 25 miles of water front within the limits of the island, and this has lately been largely increased by the acquisition of territory north of the Harlem River, all of which is available for quay and wharf purposes. The upper bay includes within its area 13 square miles of safe anchorage for large vessels, and the lower bay about 88 square miles. The average rise and fall of its tides is less than 5 feet. The lower part of the island is generally formed of sand and gravel overlying granitic rock. The upper portion, which is high on the west side, is generally rocky, the rock being granite, gneiss, mica-schist, and blue bolders, of a quality fit only for ordinary foundations. A considerable depth of black and blue mud overlies the sand, gravel, or rock of its shores. The earliest record connected with wharves and docks in the city is one dated 1654, when Daniel Litchoe, tavern-keeper, was authorised to build a dock on the strand; and in 1660 the burgomasters received permission from the Director-General to take a certain sum from shippers and owners for the erection of a pier for the accommodation of the inhabitants. The growth of the wharfage extent was gradual up to the commencement of the present century. The form of the construction of the early wharves, where hard bottom could be reached at ordinary depths, consisted in alternate cribs of wood filled with stone, and bridgeways of from 10 to 20 feet span. When holding-ground for piles could be found, piles were in many instances used. The retaining walls, or bulk-heads, were constructed of cribs, as now, but the carpentry was of the rudest kind. In 1867, the value of existing wharves, piers, and slips owned by the city of New York was estimated at nearly 16 millions of dollars. In 1870 a Department of Docks was established, who initiated a plan of permanent improvement of the water front of the city. For the new river wall, the general system adopted in 1871 was the same as that established so far back as 1782, namely, a system of narrow wharves and slips, affording the longest wharf and quay-line for the shortest extent of water frontage, combined with readiness of access. The river wall is composed of *beton* blocks weighing from 25 to 50 tons each, extending from the foundation to within about 2 feet of low water mark, and, above this level, of concrete laid in mass faced with ashlar granite masonry. The blocks are composed of—by volume—1 part of Portland cement, 2 of sand, and 5 of stone broken to pass through a 2-inch ring. These proportions have lately been changed to 1 part of cement, 2½ of sand, and 6 of broken stone. When concrete is laid loose under water, the proportions are changed respectively to 1, 2, and 3 or 4 parts. The wharves are all constructed of wood. The pier-heads are the only novel features in these structures; they are constructed of built-up columns, 20 inches by 20 inches in section, and 75 feet in length, placed in rows 12½ feet apart and 9½ feet apart in the rows. The rows are sheathed from low water up to the girders on both sides with 5-inch planking, the ends of which are protected with boiler plates. The heads of the columns are securely framed into the caps and girders. The piles used in the pier, some of which are 95 feet long, are driven in rows 8 feet apart, 5 feet apart in the row, and securely braced. The square timber is 12 inches by 12 inches in section. The columns are likewise braced with 1½-inch rods, extending from the bottom to low water.

A new addition has, within the past few years, been made to the list of Scientific Societies by the establishment of the "Association of Municipal and Sanitary Engineers and Surveyors." The inaugural meeting of this association was held in May 1873, at the Institution of Civil Engineers. The Association has since held meetings in various provincial towns, and several very interesting papers relating to drainage, sewerage, ventilation of sewers, the treatment and disposal of sewage, water supply, and cognate subjects have been contributed by its members. We shall watch with much interest the progress of this young society, and shall, on a future occasion, refer more particularly to the papers read at its meetings.

Mr. Hugh Leonard, M.I.C.E., Chief Engineer under the Government of India, has published in "Engineering" a memorandum drawn up by him on "The Weight on Foundations of Buildings." Mr. Leonard was led to enquire into this subject when commencing several new buildings in Calcutta, several buildings of recent construction being cracked, and some of them badly. The conclusions drawn, after a long series of experiments, were as follows:—"If it be desired to provide against sinking of the whole structure, the weight per foot on the earth, whatever may be the depth at which the foundation is laid, should not be more than one ton per square foot; and that, if unequal sinking cracks are to be provided against, the weight on all parts of the foundations should be equal per square foot, unless that the most heavily-loaded portion carries less than one ton per square foot. With this weight, and, of course, with lighter weights, no perceptible compression takes place, and hence no unequal settlement would occur from unequal loading." From experiments relative to the best depth for the foundation, it was ascertained that shallow foundations have the disadvantage of being affected by climatic influence, as heavy rain caused the masonry laid at a depth of 2 feet 6 inches below the surface to sink considerably. "Again it appears that foundations laid at a depth of 11 feet below the surface sink more than those laid at depths of 4 feet and 8 feet." The conclusions drawn from these experiments were that "the foundations of important buildings should be laid so deep that they cannot be affected by climate," and that they "should not be laid at a less depth than 4 feet, nor a greater depth than 6 feet." The last series of experiments undertaken were to ascertain the strength of spread foundations, and the conclusions drawn from them were that "for a pressure of one ton to the foot, on Bengal soil, the thickness at the toe of the slope should not be less than 1 foot 6 inches, and the stepping at an angle of not more than 45°.

A new market-place has recently been opened in Madrid, the materials of which were all sent out from this country. In design these markets are somewhat similar to the Halles Centrales in Paris, but are bolder, more ornamental in character, and very lofty. Structurally, the markets are composed of separate pavilions, the Mostenses Market having three, each 127 feet long and 90 feet wide, the pavilions being connected by passages, making altogether a rectangular area of 38,500 square feet. The Cebada Market covers an irregularly shaped area of 60,000 square feet, and is composed of four principal pavilions, each 119 feet long and 79 feet wide, three irregular pavilions and one lofty central dome, the pavilions being connected by covered ways, as at Mostenses. In these two structures there is nearly 4000 tons of cast- and wrought-iron. Below each market the space is utilised for cellars, the ground-floor being supported by cast-iron stanchions. The roofs are covered with galvanised corrugated iron, and there is ample ventilation by open louvres in the roofs and sides of each pavilion.

Electricity.—The following process has been recently invented in France by M. Hausen, for depositing metallic coatings upon glass or porcelain:—Sulphur is dissolved in oil and lavender and evaporated to a syrup; chloride of gold or of platinum is also dissolved in ether. The two solutions are mixed and slightly heated. They are finally evaporated to the consistence of ordinary oil-paint, and applied with a pencil to those parts of the glass or porcelain upon which it is desired to deposit the metal by the battery.

An apparatus consisting of two movable parts has been constructed by MM. Terquem and Trunnin for perforating glass with the electric spark. In the upper part of the apparatus a vertical brass rod, with ball above, point below, is enclosed in two concentric glass tubes, the small intervals being filled with colophonium. The brass point reaches through to the lower side of a horizontal glass plate attached to the tubes. In the lower part of the apparatus a right-angled brass rod is fixed, also in colophonium within a glass tube, its vertical part passing up the middle, and terminating in a point on the upper side of a horizontal glass plate resting on the tube. The tube is supported on porcelain and wood. The plate to be perforated is first coated with

oil. and put between the two horizontal plates of the apparatus. The metallic points are brought opposite each other, and the outer ends of the rods put in connection with an electric machine or induction-coil.

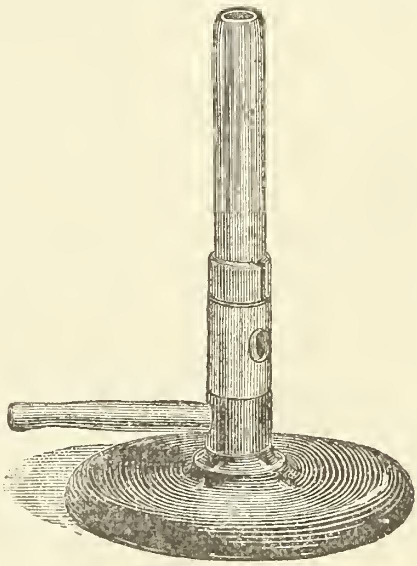
Professor W. L. Brown, of the University of Georgia, calls attention to a remarkable instance of the formation of impressions upon the human body by a lightning stroke. On the 12th of July, 1875, at about 4 p.m., a stroke of lightning fell upon a house in Americus, Ga., rendering insensible for a time four persons who were seated in one of the rooms. The two outer sides of this room, which was at a corner of the house, had each one window, and nearly opposite these on the two other sides were the chimney and the door respectively. Outside, a tree stood in front of each of the windows, and about 12 feet from the house. A third tree, a locust, stood opposite the outer corner of the room and about the same distance from the house as the others. This tree was severed by the lightning, but the other two were not affected. A young child was sitting near the centre of the room, while the mother and a lady were seated not far from the chimney, near which, and close to the wall, was another child. All these persons were rendered insensible for a time by the stroke which severed the tree, and on their recovery there were found impressed upon the bodies of them all more or less distinct images of this tree. The child near the centre of the room was impressed upon its back and exactly opposite upon its stomach. The entire tree was plain, and perfect *in toto*; every limb, branch, and leaf, and even the severed part, was plainly perceptible. It impressed the young lady upon the left hip and right leg, the mark being quite as perfect as that upon the body of the child. The mother and other child also bore less distinct impressions upon the leg. The marks are not permanent, for on August 7, the impressions were no longer distinct. It is possible to produce similar figures artificially with an electrical machine, such as the Holtz machine, capable of giving electricity of very high potential. When the poles of the latter are strongly charged and are separated to the distance of a few inches, the discharge, instead of producing a spark or brush, sometimes consists of a very small jet upon the negative, and a sort of phosphorescent glow upon the positive. The space between them, though not luminous, is the seat of a discharging action which appears to take place along definite lines, like a stream or current, and is sometimes called the dark discharge. An object placed between the poles, and in the path of the discharge, interrupts this, and destroys the glow upon the positive pole in points corresponding to the lines thus broken; and in this way there is produced an image or shadow of the interposed object, which is often strikingly distinct and perfect. In the case above described the phenomena are readily accounted for, if we suppose the thunder-cloud to have been negatively charged, and the tree to have stood in the path of the dark discharge which preceded or accompanied the lightning stroke, the action having been sufficiently intense, and the quantity of electricity great enough, to produce a visible impression upon the delicate tissues of the skin.

The *Oestereichische Landwirthschaftliche Wochenblätter* states that Dr. Virson, Superintendent of the Italian Experimental Silk-Farm at Padua, has discovered that the hatching of silk-worm eggs, of suitable age, may be accelerated by a period of ten or twelve days, and a yield of at least 40 per cent of silk-worm caterpillars secured, by exposing the eggs to a current of negative electricity from a Holtz machine for the space of eight or ten minutes. It is suggested that the same method might perhaps prove useful in hastening the germination of various seeds.

In a recent number of *Les Mondes* the Abbé Moigno directs attention to hygienic application of electricity. From his remarks it would appear that Dr. Poggioli has advised a system of "electrical gymnastics." The exact nature of these "gymnastics" is not stated, but it is mentioned that recent trials, in the presence of a committee appointed by the Prefect of the Seine, upon twenty-one school children of known physical weakness and mental debility, resulted in improved respiration and appetite, as well as in improved mental conceptions and an increase in height, weight, and chest-measurement. These beneficial effects are said to have remained three months after the

conclusion of the course. *Les Mondes* also states that the same Dr. Poggioli has succeeded in proving that electricity may be usefully employed as a sedative in nervous affections and certain acute forms of disease.

TECHNOLOGY.—In consequence of the low pressure of gas during the day-time, trouble is often experienced from the retreating Bunsen burners of the usual construction. This having repeatedly proved a source of annoyance and loss, President Henry Morton, of the Stevens Institute of Technology, was led to the following consideration of the subject:—The retreat of a burner will evidently occur whenever any part of the ascending column of mixed gas and air is moving at the orifice with a velocity less than that at which the same will burn downwards. In an ordinary burner, with its main tube of regular cylindrical bore, it is evident that the friction of the surface of the ascending column of mixed gases will cause that portion to move at a less velocity than the central part, and that currents of the nature of eddies will be developed. It will thus happen that while the central portion of the ascending column of gaseous mixture issues at a velocity much greater than that at which the material can burn downwards, and thus is quite free from any danger of retreating, the marginal portions of the column or jet of gas will be escaping at



a rate so much less that the velocity of their combustion downwards will exceed that of their upward motion, and retreat of the flame will ensue. It is well known that to secure a jet of water, or of any other fluid whose particles shall move with equal velocities in all parts, and thus avoid currents and eddies, it is only necessary to make the orifice of efflux an aperture in a thin wall. Following out the idea above indicated, President Morton made a burner of a bore rather large compared with its height, and then drew in its upper edge into the form of an open-ended thimble, so contracting the orifice of escape to about two-thirds the area of the tube, and rendering this orifice practically an opening in a thin horizontal wall or plate. The results of this modification far surpassed his anticipations. A burner thus constructed gives a perfectly non-luminous flame with gas pressures varying between 1·5 and 0·1 inch of water, and with the lowest of these pressures cannot be made to retreat by the most violent handling in the way of sudden movement or waving about in the air, even when this violence is carried to the extent of extinguishing the flame altogether. Under like conditions of pressure, a burner of the ordinary construction is made to retreat by a slight draught of air, or a very moderate amount of motion.

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I. CONSCIENCE IN ANIMALS.

By G. J. Romanes, M.A., F.L.S.

AMONG several other topics which are dealt with in an interesting article headed "Animal Depravity" that appeared in the number of this Journal for October last, the writer alludes to the question as to whether or not the rudiments of a moral sense are discernible in animals. This question I consider to be of so much importance from a psychological point of view that, although a great deal of observation which I have directed towards its enlightenment has hitherto yielded but small results, I am tempted to publish the latter, such as they are, in the hope that, if they serve no better end, they may perhaps induce some other observers to bestow their attention upon this very interesting subject.

I may first briefly state what I conceive to be the theoretical standing of the subject. At the present day, when the general theory of evolution is accepted by all save the ignorant or the prejudiced, the antecedent probability is overwhelming that our moral sense, like all our other psychological faculties, has been evolved. The question as to the *causes* of its evolution has been discussed in the "Descent of Man," and this with all the breadth of thought and force of fact so characteristic of the writings which have exerted an influence upon human thought more profound than has been exerted by the writings of any other single man—not even excepting Aristotle in Philosophy or Newton in Science. Mr. Herbert Spencer, also, has treated of this subject, and, if his wonderful "programme" is ever destined to attain completion, we may expect copious results when his great powers are brought to bear upon the "Principles of Morality." Meanwhile, however, we have ample evidence to render it highly probable that at any rate the leading causes in the development of our moral sense have had their origin in the

social instincts. Indeed, to any one who impartially considers this evidence in the light of the general theory of evolution, it must appear well-nigh incredible that so considerable a body of proof can ever admit of being overcome. Nor is this all. Not only is it true that so much success has attended Mr. Darwin's method of determining synthetically the causes which have been instrumental in evolving the moral sense,* but, long before any scientific theory of evolution had been given to the world, our great logician—following in the track of Hume (whose part in this matter has not, I think, been sufficiently appreciated), Bentham, and others—proved analytically, to the satisfaction of all competent and impartial thinkers, that the moral sense is rooted in "the greatest amount of happiness principle" as its sustaining source. In other words, John Stuart Mill, by examining Conscience as he found it to exist in Man, showed that it depends upon the very principle upon which it ought to depend, supposing Mr. Darwin's theory—elaborated, be it remembered, without any reference to Mr. Mill's analysis, and arrived at by a totally different line of enquiry—concerning the causes of its evolution to be the true one.

Stronger evidence, then, as to the physical causes whose operation has brought human conscience into being, we could scarcely expect, in the present condition of physical science, to possess. It is unnecessary, however, in this place to enter into the details of this evidence, as almost every educated person must be more or less acquainted with them. I shall therefore pass on to the next point which concerns us—namely, supposing the causes of our moral sense to have had their origin in the social instincts, where and to what extent should we expect to find indications of an incipient moral sense in animals? First, then, what do we mean by conscience? We mean that faculty of our minds which renders possible remorse or satisfaction for past conduct, which has been respectively injurious or beneficial to others.† This, at least, is what I conceive conscience to be in its last resort. No doubt, as we find it in actual operation, the faculty in question has reference to ideas of a higher abstraction than that of the fellow man whom we have injured or benefitted. In most cases the moral sense has reference to the volitions of a Deity, and in others to

* I willingly endorse the just tribute recently paid to this part of Mr. Darwin's work by Prof. Clifford :—" To my mind the simplest and clearest and most profound philosophy that was ever written upon this subject is to be found in chapters ii. and iii. of Mr. Darwin's 'Descent of Man.'"—*Fort. Rev.*, p. 794.

† For reasons which may easily be gathered from the next succeeding sentences, I omit *conscientious* ideas of what is due to *self*.

the human race considered as a whole. But if the moral sense has been developed in the way here supposed, its root-principle must be that which has reference to ideas of no higher abstraction than those of parent, neighbour, or tribe. Now, even in this its most rudimentary phase of development, conscience pre-supposes a comparatively high order of intelligence as the prime condition of its possibility. For not only does the faculty as above defined require a good *memory* as a condition essential to its existence, but—what is of much greater importance—it also requires the power of *reflecting upon past conduct;* and this, it is needless to say, appears to be a much rarer quality in the psychology of animals than is mere memory.

Thus, if Mr. Darwin's theory concerning the origin and development of the moral sense is true, we should not expect to find any indications of this faculty in any animals that are too low in the psychological scale to be capable of reflecting upon their past conduct. Whether this limitation does not exclude all animals whatever is a question with which I am not here concerned. I merely assert that if the theory in question is the true one, and if no animals are capable of reflecting upon their past conduct, then no animals can possess a moral sense, properly so-called. And from this of course it follows that if any animals can be shown to possess a moral sense, they are thereby also shown to be capable of reflecting upon their past conduct.

Again, if Mr. Darwin's theory concerning the origin and development of the moral sense is true, it is self-evident that we should not expect to find any indications of this faculty in animals that are either unsocial or unsympathetic. Supposing the theory true, therefore, our search for animals in which we may expect to find any indications of a moral sense is thus seen to be very restricted in its range: we can only expect to find such indications in animals that are highly intelligent, social, and sympathetic. Since by the hypothesis conscience requires a comparatively rare collocation of conditions for its development, we must expect to find it a comparatively rare product.

Lastly, as it is quite certain that no animal is capable of reflecting upon past conduct in any high degree, and as we have just seen that the moral sense depends upon the faculty of so reflecting, it follows that we cannot expect to find any animal in which the moral sense attains any high degree of development.

We are now in a position to draw some important distinctions. There are several instincts and feelings which,

when expressed in outward action, more or less simulate conscience (so to speak), but which it would be erroneous to call by that name. For instance, the maternal instinct, although it leads in many cases to severe and sustained self-denial for the benefit of the offspring, is nevertheless clearly distinct from conscience. The mother in tending her young does so in obedience to an inherited instinct, and not from any fear of subsequent self-reproach if she leaves her family to perish. She follows the maternal instinct, so long as it continues in operation, just as she would follow any other instinct; and it is, as it were, a mere accident of the case that in this particular instance the course of action which the instinct prompts is a course of action which is conducive to the welfare of others. An illustration will render this distinction more clear. In his chapter on the "Moral Sense" Mr. Darwin alludes to the conflict of instincts which sometimes occurs in swallows when the migratory season overtakes a late brood of young birds; at such times "swallows, house martins, and swifts frequently desert their tender young, leaving them to perish miserably in their nests." And further on he remarks—"When arrived at the end of their long journey, and the migratory instinct has ceased to act, what an agony of remorse the bird would feel, if, from being endowed with great mental activity, she could not prevent the image constantly passing through her mind, of her young ones perishing in the bleak north from cold and hunger." In other words, if we could suppose the mother bird under such circumstances to be capable of *reflecting upon her past conduct*, and as a consequence suffering an "agony of remorse," then the bird might properly be said to be *conscience-stricken*. And if we could suppose the bird, while still brooding over her young ones, to foresee the agony of remorse she would subsequently feel if she now yields to the stronger instinct by deserting her young, then the bird might properly be said to be acting *conscientiously*.

Again, mere fear of punishment must not be confused with conscience—it being of the essence of conscientious action that it should be prompted by feelings wholly distinct from fear of retaliation by the object of injury, whether by way of punishment or revenge. Conscience must be capable of effecting its own punishment if violated; otherwise the principle of action, whatever it may be, must be called by some other name.*

* Of course I recognise fear of punishment as an important factor in the *original constitution* of the moral sentiment; but, for reasons stated at the end of this article, we must, when treating of animal psychology, eliminate this factor when *conscience has become sufficiently developed to be "a law to itself."*

It is evident that conscience, as we find it in ourselves, is distinct from love of approbation and fear of disapprobation. Nevertheless, if our hypothesis concerning the development of the moral sense is the true one, we should expect that during the early phases of that development love of approbation and fear of disapprobation should have played a large part in the formation of conscience. For although, by the hypothesis, it is sympathy and not self-love that constitutes the seat of the moral sense, still the particular manifestations of self-love with which we are now concerned—viz., desire of approbation and dislike of the reverse—would clearly be impossible but for the presence of sympathy. "Mr. Bain has clearly shown that the love of praise, and the strong feeling of glory, and the still stronger horror of scorn and infamy, 'are due to the workings of sympathy.'"* I think, therefore, that in testing—by observations upon the lower animals—the truth of Mr. Darwin's theory concerning the genesis of conscience, it would be no valid objection to any satisfactory instances of conscientious action in an animal to say that such action is partly due to a desire of praise or a fear of blame. This would be no valid objection, because, in the first place, it would in most cases be impossible to say how far the implication is true—how far the animal may have acted from pure sympathy or regard for the feelings of others, and how far from an admixture of sympathy with self-love; and in the next place, even if the implication be conceded wholly true, it would not tend to disprove the theory in question. If an animal's sympathies are so powerful that, even after being reflected through self-love, they still retain force enough to prompt a course of action which is in direct opposition to the more immediate dictates of self-love, then the sympathies of such an animal are hereby proved to be sufficiently exalted to constitute the beginnings of a conscience, supposing the theory which we are testing to be the true one.

Similarly, there is an obvious distinction in ourselves between injured conscience and injured pride. But if conscience has been developed in the way here supposed, it follows that in the rudimentary stages of such development the distinction in question cannot be so well defined. Pride presupposes consideration for the opinion of others, and this in turn—as we have just seen—presupposes sympathy, which is the foundation-stone of conscience. Now it is certain that long before we reach, in the ascending scale of animal psychology, intellectual faculties sufficiently exalted to admit

* Descent of Man, p. 109 (1874). Mental and Moral Science, p. 254 (1868).

even of our suspecting the presence of an incipient moral sense, we can perceive abundant indications of the presence of pride. And forasmuch as animals that are high in the psychological scale frequently exhibit a very profound appreciation of their own dignity, we may pretty safely conclude that in no case can we expect to find indications of a moral sense in an animal without a greater or less admixture of pride.

I will now sum up this rather tedious preamble:—From Mr. Darwin's theory concerning the development of conscience, it appears to follow that the presence of this faculty in animals must be restricted—if it occurs at all—to those which are intelligent enough to be capable in some degree of reflecting upon past conduct, and which likewise possess social and sympathetic instincts. From the first of these conditions it follows, supposing Mr. Darwin's theory true, that in the case of no animal should we expect to find the moral sense developed in any other than a low degree.

There is no reason to suppose any mere *instinct* (such as the maternal) due to conscience; for an instinct acquired by inheritance is obeyed blindly, in order to avoid the uncomfortable sensation which ensues in a direct manner if it is not so obeyed,—whereas conscience enforces obedience only through a process of reflection;* the uncomfortable sensation which non-obedience entails in this case being only brought about in an indirect manner through the agency of re-presentative thought.

Although conscience in man is independent of, or distinct from, love of approbation, fear of reproach, and sense of pride, there is no reason why we should suppose conscience in its rudimentary forms to be independent of these passions. On the contrary, I think we should expect a rudimentary form of conscience to be more or less amalgamated with such passions; for long before the faculty in question has attained the highly differentiated state in which we find it to be present in ourselves, it must (by the hypothesis) have passed through innumerable states of lesser differentiation in which its existence was presumably more and more bound up with that of those more primary social instincts from which it first derived its origin. To us conscience means a massive consolidation of innumerable experiences, inherited and acquired, of remorse following one class of actions and gratification their opposites; and this massive body of

* *I. e., originally:* when once the *habit* of yielding obedience to conscience has been acquired, it becomes itself of the nature of an instinct—neglect to practise this habit giving rise immediately, or without any process of reflection, to an uncomfortable state of the mind.

experience has reference to ideas of an abstraction so high as to extend far beyond the individual, or even the community, which our actions primarily affect. No wonder, therefore, that when any course of action is being contemplated, conscience asserts her voice within us as a voice of supreme authority, commanding us to look beyond all immediate issues, inclinations, and even sympathies, to those great *principles* of action which the united experience of mankind has proved to be best for the individual to follow in all his attempts to promote the happiness or to alleviate the misery of his race. But with animals, of course, the case is different. They start with a very small allowance of hereditary experience in the respects we are considering; they have very few opportunities of adding to those experiences themselves; they probably have no powers of forming abstract ideas; and so their moral sense, rudimentary in its nature, can never be exercised with reference to anything other than concrete objects—relation, companion, or herd.

We may now proceed to answer the question already propounded, namely—Supposing Mr. Darwin's theory concerning the origin of the moral sense to be true, where among animals should we expect to find indications of such a sense? I think reflection will show that the three essential conditions to the presence of a moral sense are only complied with among animals in the case of three groups—namely, dogs, elephants, and monkeys. I need not say anything about the *intelligence* or the *sociability* of these animals, for it is proverbial that there are no animals so intelligent or more social. It is necessary, however, to say a few words about sympathy.

In the case of dogs sympathy exists in an extraordinary degree. I have myself seen the life of a terrier saved by another dog which stayed in the same house with him, and with which he had always lived in a state of bitter enmity. Yet when the terrier was one day attacked by a large dog, which shook him by the back and would certainly have killed him, his habitual enemy rushed to the rescue, and after saving the terrier had great difficulty in getting away himself.

With regard to elephants I may quote the well-known instance from the "Descent of Man":—"Dr. Hooker informs me that an elephant, which he was riding in India, became so deeply bogged that he remained stuck fast until next day, when he was extracted by means of ropes. Under such circumstances elephants seize with their trunks any object, dead or alive, to place under their knees, to prevent their sinking deeper in the mud; and the driver was dread-

fully afraid lest the animal should have seized Dr. Hooker and crushed him to death. But the driver himself, as Dr. Hooker was assured, ran no risk. This forbearance, under an emergency so dreadful for a heavy animal, is a wonderful proof of noble fidelity."*

Many cases of sympathy in monkeys might be given, but I shall confine myself to stating one which I myself witnessed at the Zoological Gardens.† A year or two ago there was an Arabian baboon and an Anubis baboon confined in one cage, adjoining that which contained a dog-headed baboon. The Anubis baboon passed its hand through the wires of the partition, in order to purloin a nut which the large dog-headed baboon had left within reach,—expressly, I believe, that it might act as a bait. The Anubis baboon very well knew the danger he ran, for he waited until his bulky neighbour had turned his back upon the nut with the appearance of having forgotten all about it. The dog-headed baboon, however, was all the time slyly looking round with the corner of his eye, and no sooner was the arm of his victim well within his cage than he sprang with astonishing rapidity and caught the retreating hand in his mouth. The cries of the Anubis baboon quickly brought the keeper to the rescue, when, by dint of a good deal of physical persuasion, the dog-headed baboon was induced to leave go his hold. The Anubis baboon then retired to the middle of his cage, moaning piteously, and holding the injured hand against his chest while he rubbed it with the other one. The Arabian baboon now approached him from the top part of the cage, and, while making a soothing sound very expressive of sympathy, folded the sufferer in its arms—exactly as a mother would her child under similar circumstances. It must be stated, also, that this expression of sympathy had a decidedly quieting effect upon the sufferer, his moans becoming less piteous so soon as he was enfolded in the arms of his comforter; and the manner in which he laid his cheek upon the bosom of his friend was as expressive as anything could be of sympathy appreciated. This really affecting spectacle lasted a considerable time, and while watching it I felt that, even had it stood alone, it would in itself have been sufficient to prove the essential identity of some of the noblest among human emotions with those of the lower animals.

If there is any validity in the foregoing antecedent reflec-

* See also HOOKER's Himalayan Journal, vol. ii., p. 333 (1854).

† I hope it is unnecessary to say that in detailing this and all the subsequent incidents, I carefully avoid exaggeration or embellishment of any kind.

tions, all who have the opportunity should make a point of observing whether any indications of conscience are perceptible in monkeys, elephants, or intelligent dogs. My own opportunities of observation have been restricted to the last of these animals alone, so I shall conclude this article by giving some instances which appear to me very satisfactorily to prove that intelligent and sympathetic dogs possess the rudiments of a moral sense.

I have a setter just now which has been made a pet of since a puppy. As he has a very fine nose, and is at liberty to go wherever he pleases, he often finds bits of food which he very well knows he has no right to take. If the food he finds happens to be of a dainty description, his conscientious scruples are overcome by the temptations of appetite; but if the food should be of a less palatable kind, he generally carries it to me in order to obtain my permission to eat it. Now, as no one ever beats or even scolds this dog for stealing, his only object in thus asking permission to eat what he finds must be that of quieting his conscience. It should be added that when he brings stolen property to me it does not always follow that he is allowed to keep it.

This same animal, when I am out shooting with him, sometimes of course flushes birds. When he does so he immediately comes to me in a straight line, carrying his head and tail very low, as if to ask for pardon. Although I speak reproachfully to him on such occasions, I scarcely ever chastise him; so it cannot be fear that prompts this demeanour.

One other curious fact may here be mentioned about this dog. Although naturally a very vivacious animal, and, when out for a walk with myself or any other young person, perpetually ranging about in search of game, yet if taken out for a walk by an elderly person he keeps close to heel all the time—pacing along with a slow step and sedate manner, as different as possible from that which is natural to him. This curious behaviour is quite spontaneous on his part, and appears to arise from his sense of the respect that is due to age.

The writer of the article on "Animal Depravity" makes the following quotation from an article of mine in "Nature" (vol. xii., p. 66):—"The terrier used to be very fond of catching flies upon the window-panes, and if ridiculed when unsuccessful was evidently much annoyed. On one occasion, in order to see what he would do, I purposely laughed immoderately every time he failed. It so happened that he did so several times in succession.—partly, I believe, in consequence of my laughing,—and eventually he became so

distressed that he positively *pretended* to catch the fly, going through all the appropriate actions with his lips and tongue, and afterwards rubbing the ground with his neck as if to kill the victim: he then looked up at me with a triumphant air of success. So well was the whole process simulated that I should have been quite deceived, had I not seen that the fly was still upon the window. Accordingly I drew his attention to this fact, as well as to the absence of anything upon the floor; and when he saw that his hypocrisy had been detected he slunk away under some furniture, evidently very much ashamed of himself."

Upon this case the author of the article on "Animal Depravity" very properly observes:—"This last point is most significant, fully overturning the vulgar notion of the absence of moral life in brutes, and of their total want of conscience." I think this observation is warranted by the facts, for although I have heard it objected that the feeling displayed by the terrier in this case was that of wounded pride rather than of wounded conscience, still, from what has been previously said concerning this distinction in the case of animals, it will be seen that in this instance it is not easy to draw the line between these two sentiments.

The following instances, however,—all of which occurred with the terrier just mentioned—are free from this difficulty.

For a long time this terrier was the only canine pet I had. One day, however, I brought home a large dog, and chained him up outside. The jealousy of the terrier towards the new-comer was extreme. Indeed I never before knew that jealousy in an animal could arrive at such a pitch; but as it would occupy too much space to enter into details, it will be enough to say that I really think nothing that could have befallen this terrier would have pleased him so much as would any happy accident by which he might get well rid of his rival. Well, a few nights after the new dog had arrived, the terrier was, as usual, sleeping in my bed-room. About 1 o'clock in the morning he began to bark and scream very loudly, and upon my waking up and telling him to be quiet he ran between the bed and the window in a most excited manner, jumping on and off the toilette-table after each journey, as much as to say—"Get up quickly; you have no idea of what shocking things are going on outside." Accordingly I got up, and was surprised to see the large dog careering down the road: he had broken loose, and, being wild with fear at finding himself alone in a strange place, was running he knew not whither. Of course I went out as soon as possible, and after about half-an-hour's

work succeeded in capturing the runaway. I then brought him into the house and chained him up in the hall; after which I fed and caressed him with the view of restoring his peace of mind. During all this time the terrier had remained in my bed-room, and, although he heard the feeding and caressing process going on downstairs, this was the only time I ever knew him fail to attack the large dog when it was taken into the house. Upon my re-entering the bed-room, and before I said anything, the terrier met me with certain indescribable grinnings and prancings, which he always used to perform when conscious of having been a particularly good dog. Now I consider the whole of this episode a very remarkable instance in an animal of action prompted by a sense of *duty*. No other motive than the voice of conscience can here be assigned for what the terrier did: even his strong jealousy of the large dog gave way before the yet stronger dread he had of the remorse he knew he should have to suffer, if next day he saw me distressed at a loss which it had been in his power to prevent. What makes the case more striking is, that this was the only occasion during the many years he slept in my bed-room that the terrier disturbed me in the night-time. Indeed the scrupulous care with which he avoided making the least noise while I was asleep, or pretending to be asleep, was quite touching,—even the sight of a cat outside, which at any other time rendered him frantic, only causing him to tremble violently with suppressed emotion when he had reason to suppose that I was not awake. If I overslept myself, however, he used to jump upon the bed and push my shoulder gently with his paw.

The following instance is likewise very instructive. I must premise that the terrier in question far surpassed any animal or human being I ever knew in the keen sensitiveness of his feelings, and that he was never beaten in his life.* Well, one day he was shut up in a room by himself, while everybody in the house where he was went out. Seeing his friends from the window as they departed, the terrier appears

* A reproachful word or look from me, when it seemed to him that occasion required it, was enough to make this dog miserable for a whole day. I do not know what would have happened had I ventured to strike him; but once when I was away from home a friend used to take him out every day for a walk in the park. He always enjoyed his walks very much, and was now wholly dependent upon this gentleman for obtaining them. (He was once stolen in London through the complicity of my servants, and never after that would he go out by himself, or with anyone whom he knew to be a servant.) Nevertheless, one day while he was amusing himself with another dog in the park, my friend, in order to persuade him to follow, struck him with a glove. The terrier looked up at his face with an astonished and indignant gaze, deliberately

to have been overcome by a paroxysm of rage; for when I returned I found that he had torn all the bottoms of the window-curtains to shreds. When I first opened the door he jumped about as dogs in general do under similar circumstances, having apparently forgotten, in his joy at seeing me, the damage he had done. But when, without speaking, I picked up one of the torn shreds of the curtains, the terrier gave a howl, and rushing out of the room, ran up stairs screaming as loudly as he was able. The only interpretation I can assign to this conduct is, that his former fit of passion having subsided, the dog was sorry at having done what he knew would annoy me; and not being able to endure in my presence the remorse of his smitten conscience, he ran to the farthest corner of the house crying *peccavi* in the language of his nature.

I could give several other cases of conscientious action on the part of this terrier, but as the present article is already too long I shall confine myself to giving but one other case. This, however, is the most unequivocal instance I have ever known of conscience being manifested by an animal.

I had had this dog for several years, and had never—even in his puppyhood—known him to steal. On the contrary, he used to make an excellent guard to protect property from other animals, servants, &c., even though these were his best friends.* Nevertheless, on one occasion he was very hungry, and in the room where I was reading and he was sitting, there was, within easy reach, a savoury mutton chop. I was greatly surprised to see him stealthily remove this chop and take it under a sofa. However, I pretended not

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turned round, and trotted home. Next day he went out with my friend as before, but after he had gone a short distance he looked up at his face significantly, and again trotted home with a dignified air. After this my friend could never induce the terrier to go out with him again. It is remarkable, also, that this animal's sensitiveness was not only of a selfish kind, but extended itself in sympathy for others. Whenever he saw a man striking a dog, whether in the house or outside, near at hand or at a distance, he used to rush to the protection of his fellow, snarling and snapping in a most threatening way. Again, when driving with me in a dog-cart, he always used to seize the sleeve of my coat every time I touched the horse with the whip.

* I have seen this dog escort a donkey which had baskets on its back filled with apples. Although the dog did not know that he was being observed by anybody, he did his duty with the utmost faithfulness; for every time the donkey turned back its head to take an apple out of the baskets, the dog snapped at its nose; and such was his watchfulness, that, although his companion was keenly desirous of tasting some of the fruit, he never allowed him to get a single apple during the half-hour they were left together. I have also seen this terrier protecting meat from other terriers (his sons), which lived in the same house with him, and with which he was on the very best of terms. More curious still, I have seen him seize my wristbands while they were being worn by a friend to whom I had temporarily lent them.

to observe what had occurred, and waited to see what would happen next. For fully a quarter of an hour this terrier remained under the sofa without making a sound, but doubtless enduring an agony of contending feelings. Eventually, however, conscience came off victorious, for, emerging from his place of concealment and carrying in his mouth the stolen chop, he came across the room and laid the tempting morsel at my feet. The moment he dropped the stolen property he bolted again under the sofa, and from this retreat no coaxing could charm him for several hours afterwards. Moreover, when during that time he was spoken to or patted, he always turned away his head in a ludicrously conscience-stricken manner. Altogether I do not think it would be possible to imagine a more satisfactory exhibition of conscience by an animal than this; for it must be remembered, as already stated, that the particular animal in question was never beaten in its life.*

II. NATURE'S SCAVENGERS.

IN these days of Sanitary Reform it may be interesting to examine what is the real state and value of existing natural arrangements for the removal of nuisances, and for the disinfection of the waters and the atmosphere. The subject may, fortunately, now be discussed without calling forth those strong expressions of affected disgust with which it would have been greeted not many years ago. It has a very obvious practical bearing: we have to consider what is the actual utility of Nature's Scavengers,—in how far we may trust to their action,—when and where we should assist and cherish them, and under what circumstances we should seek to supersede them altogether. Nor is the question without a speculative interest. The efficiency and completeness, or the opposite qualities of Nature's agencies for dealing with refuse, may throw some valuable cross-lights upon the origin of species, and indeed upon the whole debate

* This latter point is most important because, although the moral sentiment in its incipient stages undoubtedly depends in a large measure upon fear of punishment, still in its more developed state this sentiment is as undoubtedly independent of such fear (Cf. Bain, "Mental and Moral Science," pp. 456-9, 1875); and forasmuch as in our analysis of animal psychology we can be guided only by the study of outward actions, and forasmuch as the course of action prompted by direct fear of punishment will nearly always be identical with that prompted by true conscience, it is of the first importance to obtain cases such as the above, in which mere dread of punishment cannot even be suspected to have been the motive principle of action.

now pending between the old and the new schools of Natural History.

We will at once begin by examining what living agencies exist for the removal of carrion, excrement, and other putrid or putrescent matters on land. Among the Mammalia we find no inconsiderable number of species which feed upon decomposing animal matter. The Felidæ certainly prefer living prey, but in case of need even the lion of Algeria will compromise the dignity with which he has been invested by closet-naturalists by devouring putrid carcases, and the very *immondices* which generally accumulate outside the walls of a town. The true panther (*Felis pardus*), also, in case of need, preys upon carrion, and will even dig up and devour dead bodies. The Bengal tiger is said to eat the dung of the rhinoceros. But the genuine carrion-eaters are the Canidæ —the jackal, the wolf, and the domestic dog. These animals appear to like their food tainted; they will roll themselves upon a putrid carcase, and even when well fed they will greedily devour human excrement. No species or variety is more given to this loathsome diet than the King Charles spaniel. But when such substances are eaten by any animal we have to ask whether the nuisance is really overcome, or merely altered in its form? The latter view is much more consonant with truth. The secretions and excretions of a carrion-feeder are in quality little better than the refuse eaten. A certain quantity is, indeed, consumed in the body of the filth-devourer, and makes its reappearance in the shape of inorganic compounds, such as carbonic acid, watery vapour, and ammonia. But all that is given off in complex organic combinations is noisome in the extreme, and rapidly passes into a state of energetic putrefaction. The disgusting odour of the wolf, the hyæna, the vulture, and even of the domestic dog when stretched out before a cheerful fire, cannot escape recognition. These creatures, therefore, occupy an intermediate rank. As scavengers they are often useful, but not perfect. They do not propagate and increase nuisance, but neither do they entirely suppress it when thrown in their way.

There is, however, one instance, at least, of perfect scavenging among beasts of prey. We refer to the habit of the domestic cat in scraping earth over her excrements. This custom does not extend to the whole even of the Felidæ, but it re-appears in a rudimentary—or perhaps obsolescent—form in the dog, who generally gives two or three random scrapes or kicks with his hind legs on such occasions —a ceremony which, as now performed, can be of no use either to the animal himself or to any other creature.

In how far the omnivorous rodents—such as rats and mice—may be regarded as scavengers is somewhat doubtful. The best claim may be made for the grey or Norway rat (Hanoverian rat of Waterton), who is very fond of taking up his abode on the premises of knackers, bone-boilers, &c., and sometimes succeeds in penetrating into family vaults for the purpose of gnawing human remains. The black rat and the common mouse may occasionally devour putrid animal matter under the pressure of scarcity, but it by no means forms their ordinary or favourite diet. Still, animals of this tribe are, at the best, very unacceptable scavengers, from considerations which will be fully detailed when we come to speak of carrion-flies.

Among birds we find two distinct main groups of carrion-eaters—the vultures and the crows. Both execute their task in exactly the same manner as the Canidæ. They exhale from their skins the same detestable odour, and their dung is offensive in the extreme; but as they are not given to injure man—either in his person or to any appreciable extent in his property—they are, as scavengers, far superior to wolves, jackals, hyænas, panthers, and rats, and they may justly claim immunity from persecution, or even positive protection. Another bird, though very promiscuous in its diet, has some title to be regarded as a scavenger: we refer to the domestic duck, which will feast greedily upon putrid matter, the remains of its own kindred not excluded. Even tumours, the dressings from ulcers, and other the foulest *rejectamenta* of hospitals, are not disdained by these birds, which yet, in England, serve for the very type of all that is delicate.

Among reptiles we are unable to point out any species of scavenging habits. Although the great majority of lizards and serpents are purely carnivorous, they seek invariably living prey. This the writer was able to establish most conclusively, as far at least as European species are concerned, by a long series of observations made upon a numerous colony of these creatures, confined in a pit somewhat resembling a melon-frame, but filled with peaty earth, stones, and bushes of heath and of *Ledum palustre*. Small dead animals, such as mice, shrews, small birds, &c., if thrown into the pit, were completely disregarded by the snakes; but if a live mouse was thrown in, all the venomous inmates were up in arms in a moment, until one of their number had given the fatal bite. On one occasion only a large male viper, who had been very restless for some days, was observed carrying a limb of a dead bird about in his mouth;

he made, however, no attempt to swallow it. The alligators of the western hemisphere are said to bury their prey in the mud of the beds of rivers for some time before they devour it—a notion which is strenuously combatted by Waterton; but this habit, even if verified, scarcely entitles them to be regarded as regular eaters of carrion.

The amphibians do not appear to consume any putrescent or putrid matter, whether of animal or vegetable origin.

The more widely spread are scavenging habits among fishes, a very large proportion of which partake almost indifferently of organic matter, whether living or lifeless, fresh or decomposing.

It is, however, not amongst vertebrate animals, but among the Articulata, and especially among insects, that we find the most active and powerful scavengers. Here immense numbers and great voracity more than compensate for smallness of size.

The most numerous and the most efficient of insect scavengers are to be found in the so-called "order" Coleoptera.* Amongst these insects it is easier to particularise those which do not, wholly or in part, subsist upon vegetable and animal refuse than those which do. We shall, therefore, not furnish a catalogue of scavenging families and genera, but merely point out the most important. Perhaps the highest rank may be claimed by the Silphidæ. These insects not merely feed upon carrion, both when larvæ and in the adult state, but one of their genera—the *Necrophori*, or sexton-beetles—bury putrescent animal matter as food for their young. Wherever they find a small dead animal—mouse, frog, bird, &c.—or a fragment of some larger carcase which has been neglected by hyænas, jackals, vultures, and the like, they deposit their eggs therein, and then dig away the earth from below it, and cover it up. Their manner of proceeding has been so well described in various works on Natural History† that it need not be repeated here. But the quantity of matter which they thus inter deserves particular attention. A single sexton-beetle has been known to bury a mole forty times its own weight. Four beetles have been seen burying a crow, which would certainly exceed their united weights in a still higher ratio. Thus not only is nuisance prevented, but the earth is enriched with an

* It must be ultimately admitted that the divisions Coleoptera, Lepidoptera, &c., are of far higher rank than orders. The coleopterous group Adephaga (Clairville) seems to be an order, equal in rank to Carnivora amongst mammals.

† For instance, RENNIE's Insect Architecture, p. 233, and KIRBY and SPENCE, ii., 350. See also Act. Acad. Berolin, 1752.

excess of matter more than can be consumed by the larvæ. Nor is the mischief of putrefaction merely deferred or altered in form, as in the case of many carrion-feeders, since the excrements of the larvæ are absorbed by the earth, as well as the gases and vapours resulting from the decomposition of the dead body.

Before we pronounce any scavenger perfect we must be sure that he confines himself solely to his useful, though repulsive task, and does not go about bedaubed with filth, disseminating the seeds of putrefaction, and probably of disease. Tried by this test the sexton-beetle is justified. He does not intrude into our dwellings, settle on our food, and buzz about our persons, contaminating whatsoever he touches. He removes, without propagating, nuisance, and the removal is not attended with any drawback or set-off. We must therefore declare him an admirable, unimpeachable scavenger, wishing him every success in his operations, and full immunity from the competition of quacks and bunglers. But with all these good qualities—perhaps we must even say because of them—he barely holds his own in the struggle for existence, and seems to us to be decidedly decreasing in numbers. An animal which feeds on one kind of food only is naturally at a disadvantage if it has to compete for the means of existence against omnivorous species. The *Necrophorus*, feeding only on carrion, is "underbidden" by creatures which can feed upon almost any kind of organic matter, and which are perfectly ready to deposit their ova in fresh flesh if they can find nothing already tainted. This is, we submit, a crucial case, deciding the comparative merits of the new and of the old natural history, and as such we shall have to refer to it again. No less does it exclude certain applications of the doctrine of "Natural Selection" made by political economists. We have still to notice the extreme limitation of the burying propensity. The other genera of the Silphidæ, though preying upon carrion* and depositing their eggs in the same material, do not bury, and are consequently of much less value as removers of nuisances. Now, if the burying propensity be an instinct especially implanted in these insects in order to preserve the air from taint, why is it restricted to one small group—neither rich in species nor in individuals—among an extensive family of carrion-feeders? Might we not rather

* Some of the species of *Silpha* attack living prey. MacLeay states that *Silpha 4-punctata* ascends oaks in pursuit of caterpillars. We have captured it on oaks in Dunham Park, near Manchester, under circumstances which decidedly favour this view.

expect that all the Silphidæ would bury, whenever, at least, they met with a carcase of suitable magnitude and in a fitting locality? If, therefore, we accept the point of view of the old natural history, we are forced to admit that an instinct which would have been beneficial both to these insects and to the world at large has been inexplicably withheld from them. Beneficial, we say, to the insects themselves, because larvæ in a piece of buried carrion are—all things considered—safer and more likely to reach maturity than if the carrion had been allowed to remain on the surface of the earth. In this latter case the larvæ are exceedingly liable to be picked out and consumed by birds, or the whole piece of carrion may at any moment be devoured by some passing dog, wolf, or hyæna. Beneficial, also, to the world at large—for if the burial of offensive matter secures the air from taint, it is surely important that all such matter should be buried, and not merely a small part.

We cannot especially examine the families of Sphæridiidæ, Histeridæ, and the vast group of Brachelytra, rove-beetles or "devil's coach-horses," containing nearly 800 British species. All these derive a part, at least, of their support from decaying animal and vegetable matter, and may be considered as good scavengers of the second rank—*i.e.*, devourers of filth which do not disseminate pollution. But we must turn to a more remarkable and interesting tribe of scavengers, the Saprophagous Lamellicornes of MacLeay, of which the common dung-beetle may be taken as the type.

If the reader, when taking a stroll in the fields during the spring or early summer, turns over with his stick a deposit of horse- or cow-dung, he will—except accustomed to entomological explorations—be astonished at the number and variety of living beings presented to his view. If the dung lies upon soft ground, and is neither too recent nor too old and dried up, he may often find beetles of a dozen or more species, all making arrangements for keeping up the circulation of matter. Whilst the dung itself is tenanted by *Staphylini*, *Histers*, *Sphæridiæ*, and their maggots, in the ground beneath he will generally see several round holes, varying from a few lines to nearly an inch in diameter, and extending a considerable depth into the ground.*

* The burrows of *Typhæus vulgaris*, the three-horned dung-beetle, are bored exceedingly neatly. I cannot help suspecting that, after ground has once been broken, the three thoracic horns of this species—which all point forwards, and are nearly parallel to each other—play a part in the operation. They are, indeed, much less developed in the female than in the male, but the male is so frequently found in the shaft that it seems but reasonable to suppose that he takes a share in the work of excavating.

Down these he will often see beetles quietly escaping. These holes are the burrows of different species of dung-beetle, into which they convey a quantity of excrement, and in it deposit their eggs. Here, again, as in the case of the sexton-beetle, is perfect scavenging. A large part of the dung—more than is merely required for the wants of the young grub—is carried down into the earth, where also the excrements of the maggot are retained and disinfected. In one single patch of dung, therefore, we may find examples both of perfect and of very imperfect scavenging; the former affected by different species of *Geotrupes*, *Aphodius*, *Typhœus*, &c., as just described, and the latter by *Staphylini*, Histeridæ, &c., which merely devour the dung where it lies, without carrying it down into the soil. Here then, again, the question may be asked—If to bury putrid matters be an instinct specially implanted in, *e.g.*, the Geotrupidæ, in order to preserve the atmosphere from taint, why has it not been extended to all the dung-feeding species? Even among those which do bury excrementitious matters we find many gradations in their mode of working. The *Geotrupes* and their near allies, in Britain and Central Europe, as we have seen, excavate a shaft directly under the dung, and carry down as much as they think requisite. The species of *Ateuchus*—the sacred beetle of the Egyptians—make up a ball of dung, in which they enclose an egg, and push or roll it along to a hole which they have either dug or selected with some little adaptation. *Pachysoma æsculapius* does not make up a ball, but deposits its egg in a hole, to which it brings dried dung in pieces. *Coprobius volvens*—the "tumble-dung" of North America—is a ball-roller. But among the *Coprobii* of Brazil one alone (*Coprobius carbonarius*) buries dung. These facts, if fairly weighed, seem to show that the practice of burying filth is not a primordial instinct, but has been acquired in the course of successive generations, and has taken various developments in various groups. We find, also, among these dung-buriers, some species which agree completely in their food and in their habits with the *Necrophori*. Whilst the other members of the great and splendid South-American genus *Phanæus* burrow under dung precisely like our British *Geotrupes*, *Phanæus melon* mines under dead fish, and *P. nigro-violaceus* and *sulcatus*—according to Prof. Westwood—dig holes beneath dead serpents, and bury them in a few hours.

We have next to point out an important shortcoming in the dung-eating, and even in the dung-burying, beetles. What the latter class undertake is done to perfection, but

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they decline some of the most important duties of a scavenger. They eagerly attack the dung of the ruminants and of the Equidæ. But such excrement, after all, consists chiefly of comminuted vegetable fibre, and—except artificially collected together in large quantities, where it may enter into fermentation or pollute rivers—it can scarcely be considered as markedly injurious to the health of man or of other animals. The dung of omnivorous beasts, as the hog, they do not affect; that of the Carnivora and of man they seem ordinarily to avoid. The *Hybosori* of Brazil, indeed, according to Westwood, frequent human excrement, but without burrowing in it, and a son of the writer once captured *Onthophagus nuchicornis* on the same material in Highgate Wood.

Generally speaking we are, then, warranted in concluding that the more offensive and dangerous any kind of excrement, the less are the Saprophagous Lamellicornes disposed to undertake its removal, leaving the worst cases to creatures who deal with them in a very unsatisfactory manner.

Passing in review the remaining "orders" of insects, we find no true scavenging species in the Lepidoptera. Some of the most beautiful butterflies will, however, occasionally indulge in a taste for abominations. *Apatura Iris* will come down from his airy flights to sip the putrid moisture oozing from a dead rat or weasel. The splendid *Papilios* and *Ornithopteras* of warmer climates will, in like manner, stoop to human excrement and carrion. This is a curious instance of animals purely herbivorous in their earlier stages becoming carnivorous, or rather omnivorous, when mature. The clothes'-moth and its congeners, indeed, feed upon dead animal matter, but only upon such as does not readily enter into decomposition and become offensive.

Among the Hymenoptera, the ants prey upon a great variety of substances, living and lifeless. They by no means refuse the bodies of small animals, birds, &c., which fall in their way, and may thus—by consuming matter that might otherwise be left to putrefy—rank as indirect scavengers. But we have never met with any authenticated case of their devouring matter already putrid, and certainly not excrement. Among ants we meet with the only established instance among the lower animals of a formal burial of the dead.* Wasps and hornets carry off fragments of meat from the butchers' shops, but they avoid carrion. Excrement they hold in great horror, and it is interesting to watch

* Journal of the Linnean Society, v. 217.

the nicety with which a wasp snatches away flies from such nuisances without soiling its feet or wings.

Among the Orthoptera, Neuroptera, Homoptera, and Hemiptera there are no true scavengers. The Diptera, on the other hand, feed to a very large extent upon carcases and fœcal matter, and many of them may at first sight be taken for scavengers of great efficiency. There are very few of the Muscidæ—the family including the common house-fly—which do not, at one part of their lives, feed upon dung, carrion, or decomposing vegetables. Thus the blow-flies, *Lucilia Cæsar* and *Calliphora vomitaria*, like the *Silphæ* and *Necrophori*, deposit their eggs in dead animal matter. But, unlike the beetles just mentioned, they neither bury the substances containing their eggs nor do they restrict their attacks to tainted meat. Their ova and larvæ possess a remarkable power of setting up and intensifying putrefaction in any animal matter with which they come in contact. Even living animals are not exempt from the attacks of these and allied species, which take every opportunity to lay their eggs in open wounds or abrasions of the skin. These flies, also, after being in contact with the most loathsome substances, settle upon man's person and on his food. Now how minute a portion of putrid blood, pus, &c., may set up dangerous changes in articles of diet, or may serve as the vehicle of disease, we do not exactly know, but we have reason to believe that exceedingly small quantities will suffice. Very similar charges must be brought against the house-fly (*Musca domestica*), the privy-fly (*Anthomyia canicularis*), and the blood-sucking *Stomoxys calcitrans*. All these, and many more indeed, at once consume nuisances and propagate them. The proboscis of a *Stomoxys* thrust into our flesh has perhaps, a moment before, been saturated with morbid matter, and the result of its bite may be carbuncle. Ophthalmia is certainly propagated by flies, and a closer examination will doubtless prove that variola, typhus, black vomit, scarlatina, and zymotic disease generally are spread about in the same way. Hence it is of the greatest importance that the dejections of patients suffering from such diseases, the bodies of the dead, and every substance which can have imbibed the morbific matter, should be treated in such a manner that these minute harpies may be prevented from settling and feasting upon it. The open cesspools attached to privies in country places are often one wriggling mass of maggots, and seem to call loudly for a liberal dose of carbolic sulphite, or some other enemy to low forms of life. It is in such localities that the attack must be made.

To destroy these pseudo-scavengers—who in reality, like the "pushing" quacks of whom they are the type, intensify the ills which they seem to cure—we must poison their pabulum. One of the perils of sewage irrigation is, that it gives great facilities for the multiplication of flies in the ever-moistened earth. The claims of the mosquito and its allies to be regarded as purifiers of the rivers, and of the house-fly to rank as the purifier of the air, we shall discuss below. Reviewing, then, the character of Dipterous dung and carrion devourers, we are tempted to ask—What is, after all, their especial mission? Are they adapted to remove putrid matter, or to propagate putrefaction and disease? We fear the balance of evidence is in favour of the latter view, and that one great class of nature's scavengers does more harm than good. The significance of this is apparent. But the most remarkable fact is that these pseudo-scavengers, the Diptera, are more successful than the genuine perfect scavengers among the Coleoptera. They are ten-fold, perhaps a hundred-fold, more numerous than the Silphidæ and the Saprophagous Lamellicornes. Instead of receding before the advance of civilisation and the increase of human population, they seem—like rats, mice, bugs, cockroaches, &c.—to grow upon us, and may yet constitute a danger more serious than some of us imagine.

Several Crustaceans will be mentioned among aquatic scavengers. The land-crabs, however, of which there are several species in warm climates, are given to prey upon dead animals. Some of them have been even known to devour human bodies which had been negligently buried.

The snails and slugs, to which we shall have to recur as consumers of vegetable refuse, occasionally prey upon dung. We have frequently, during entomological rambles in the early morning, seen the large common black slug feasting heartily upon human excrement. To what other species this habit extends we are unable to say, but we commend the fact to the careful consideration of all lovers of *escargots*.

Vegetable refuse probably contributes as much to the unhealthiness of a district as animal substances, being generally much more abundant. Its removal is very unequally provided for, and is scarcely even attempted by vertebrate animals. Decayed and decaying timber is broken up and consumed by myriads of insects: in cold climates by the larvæ of the goat-moth, the wood-leopard, the stag-beetle*

* We have never known a perfectly sound tree attacked by this fine insect (*Lucanus cervus*), and we there ore question the justice of ranking him among the enemies of the gardener.

and its congeners, the Longicornes, termed wood-beetles by pre-eminence, and many others: in warmer climates the task is taken up by the splendid Buprestidæ, the Dynastidæ (such as the elephant and Hercules beetles), and some species of the dung-burying tribes, as well as by the numerous and gigantic Longicornes of those regions, such as the "harlequin." The *Termites*, or white ants as they are improperly called, also engage in the task of removing decayed wood, but as they are equally prone to consume sound timber they must be regarded as scavengers of the lowest or objectionable type, like the carrion-flies.

Fallen leaves are not efficiently consumed by any class of animals, and where they have accumulated in quantities they may still be found in the next season in various stages of decomposition. Slugs and snails are considered as the scavengers for *effete* vegetable matter, but they occupy themselves chiefly with eating sound leaves and fruits, and hence they must rank in the lowest class. To some extent decayed leaves are pulled into the ground by earth-worms, and when far advanced in decomposition are attacked by the Brachelytra, and others of the many insects that help to dispose of the dung of herbivorous animals. But there is evidently no systematic removal at all commensurate with the occasion.

That the waters are polluted by their inmates, animal and vegetable, needs little formal proof. Their excrements, their cast-off skins, their decaying bodies, their abortive ova, all contribute a large supply of offensive and injurious matter. Nor are the dwellers on land excluded. From the autumn leaves which fall into the forest-pool,—from the decaying tree-trunk, fallen into the river, and gradually yielding up its soluble constituents or the products of its decomposition, —onward to the drainage of the cess-pool or the churchyard winning its way into the village well and to the sewage of some "closetted" town, contaminating the main arteries of a kingdom,—we find everywhere the same class of results. The waters are tainted by the residues of animal and vegetable life. The contamination is going on all around, but what are the means for its abatement? Has Dame Nature issued a "Rivers Pollution Commission," and, if so, is it more efficient than that lately sent out by our gracious Sovereign? Some self-styled authorities on this question fall little short of declaring that there is no natural process by which water, once contaminated with animal excrement or with putrid carcases, can be purified. With all cleansing operations, real or imaginary, carried on by lifeless agencies we have here no concern. There are certain water-plants

which are powerful purifiers. Like all the higher forms of vegetable life they decompose carbonic acid under the influence of light, and evolve oxygen, by which the impurities are literally burnt up. Among such plants the common duckweed holds no undistinguished place.

Animal scavengers exist, also, which eat up the putrid or putrescent matter. Some work of this nature is done by aquatic birds—by none more eagerly than by the common duck. Any dead body floating in the water, whether that of a mammal, bird, or fish, is greedily gobbled up by these unclean feeders. We doubt whether any reptile or amphibian can be called a purifier of the water. The frog is commonly supposed to exercise this function, and its presence in wells is therefore viewed with approval. Now there is no doubt that it consumes the larvæ and ova of insects,—matter by no means desirable to be introduced into the human stomach, —and thus to some extent improves the water; but it seeks living prey, and we have never known it consume any dead or putrid matter, whether of animal or vegetable origin. Fishes—for example, the eel—and Crustaceans are diligent consumers of dead bodies floating in or sunk under water. The shrimp, by its performances of this kind, has earned the title of the "scavenger of the ocean." But we have no reason to believe that either fish or Crustacea can deal with those minute, almost pulpy, fragments of decomposing matter which form so large a proportion of the pollutions of lakes, rivers, and even seas. Here, as far at least as fresh water is concerned, the task is taken up by insects. It is well known that many Diptera and Neuroptera, though winged creatures, when arrived at maturity, begin their life in the waters. Many of these, when larvæ, feed upon this very pulp of decomposing animal and vegetable matter to which we have just referred. To this class belong the gnats and mosquitoes. It is accordingly maintained that the annoyance which they occasion when mature is compensated, and even outweighed, by their sanitary services when larvæ. Without their aid, it is said, certain districts in tropical climates would be absolutely uninhabitable on account of malaria. We must confess to no small amount of scepticism on this subject. We find, first, mosquitoes very prevalent where malaria is malignant. Here, then, is a proof that the mosquitoes, if they contribute anything to the health of a district, fall incalculably short of what is requisite. Again, we find them swarming in countries where malaria is unknown, *e.g.*, in Lapland, and where, from the nature of the climate, it seems not probable to arise. There are, also,

districts in tropical South America exceptionally healthy, and, by a curious coincidence, free from mosquitoes and other insects of similar habits. Yet more, there are places —now and always healthy—where mosquitoes were formerly unknown, but where they have now been introduced, probably by shipping, and appear to be gaining ground. Surely these considerations go far to disprove the notion of mosquitoes being important sanitary agents, specially destined for the purification of pools and rivers. But there is yet a further objection, applying more or less to all natural scavengers which remove nuisances by using them as food. Granting that the mosquito in its larva state consumes a certain quantity of putrid or putrescent matter, there is a very considerable set-off. Its excrements, its cast-off skins, and finally its body when dead, are matter no less offensive and dangerous than the food which it has eaten. An insect when dead is, size for size, as decided a nuisance as the remains of a larger animal. That pestilence has followed the death and decomposition of large armies of locusts is a well-known fact. We cannot believe that the millions of dead mosquitoes can have any much more beneficial effect.

We are not aware of any Coleopterous, Hemipterous, Homopterous, Orthopterous, Hymenopterous, or Lepidopterous insect that—either in the larva state or after reaching maturity—takes part in the purification of waters. Many species of the two former orders are aquatic, but they select living prey.

Water-snails and other Mollusca may be ranked among the scavengers of the water, subject always to the limitation that they, in turn, produce a certain amount of pollution. They have, at any rate, this advantage over the mosquito and its Dipterous allies, that they do not issue from the waters and devote the rest of their lives to the annoyance of mankind.

But besides solid suspended nuisances, in a coarser or finer state of subdivision, polluted water contains a certain amount of organic matter in a state of actual solution, such as the extractive matter of plants and the fluid secretions and excretions of animals. This soluble impurity cannot be eaten out of the water like suspended matter, and the bulk of the above-mentioned agencies are, therefore, useless for its removal. Fresh-water Mollusca may, perhaps, purify such waters to a small extent, but the oxygen liberated from aquatic plants is far more widely efficacious.

But the worst remains to be told. So long as a stream of water is only moderately impure the plants and animals

above mentioned exercise a very beneficial action upon its condition, and if they do not succeed in bringing it to a state suitable for domestic use, they, in millions of cases, prevent it from tainting the air. But it is said, and not without a foundation of truth, that if the pollution becomes excessive Nature's scavengers either beat an ignominious retreat or die at their posts. We must admit that there are in England, and probably in other populous countries, waters so contaminated as to be habitable by nothing higher than Fungi and Infusoria. A certain French physicist has even proposed the presence of certain species of plants and animals, as furnishing a scale by which the sanitary condition of a river might be approximately estimated. It may therefore be interesting to give a case in point, which was observed and carefully studied by the present writer in the spring and summer of 1868. With a view of throwing light upon a scheme in contemplation for purifying the refuse waters of a large dye-works, we visited a branch of the Calder and Hebble Navigation, which extends from the bottom of the town of Halifax down to the village of Salter Hebble, about $1\frac{1}{2}$ miles off, where it joins the main trunk of the Calder. The descent is so steep that the canal is merely a series of pools separated by locks, and at its origin in the town it is fed not with water, but with sewage pumped up from the shallow River Hebble, which receives the domestic and manufacturing refuse of the town.* Having inspected the basin, where the water was very foul and turbid, and emitted a disgusting odour, we walked along the towing-path, carefully noting the phenomena presented. For the first two or three locks there was no material change; no animal life could be detected in the water, and no plants were seen except sewage fungus, which had here and there attached itself to the stonework. At last, when about half the descent had been accomplished, water-weeds began to make their appearance—at first few, and by no means flourishing. They rapidly became more numerous, both in individuals and in species, and more luxuriant. The water exhibited a corresponding improvement in colour and odour, and when we had arrived at the last pool—above Salter Hebble—we recognised a variety of water-insects. The common whirligig beetle (*Gyrinus natator*) was spinning round amidst the duckweed; an *Acilius* and several *Colymbetes* plunged into deep water as we approached, and the water-scorpion was disporting himself on the surface. None of these insects

* We understand that these arrangements have been since altered.

contribute at all to the purification of the water, being carnivorous; but their presence was a proof of the existence of other forms of insect-life, on which they could prey. The canal, it must be added, was very sparingly used, and its successive pools might be regarded as a series of subsidence tanks. Hence it appears that subsidence and exposure to air alone will bring excessively polluted water to a state which permits of the existence of aquatic vegetation and of insect-life. This point once reached, a further and rapid improvement is effected by these natural agencies. In a flowing stream, or in a canal at one uniform level and stirred up by constant traffic, these conditions would not occur. Under ordinary circumstances, therefore, it must be admitted that natural scavenging arrangements fail in case of excessively polluted waters. One error must be here carefully guarded against. Trees and shrubs growing in or near the water have no direct part in its purification. The oxygen given off from their leaves mixes with the atmosphere, and only acts upon the surface of the water; but plants whose leaves are on or underneath the surface give off oxygen, which in its nascent state comes in contact with the suspended or dissolved impurities, and effects their combustion.

We have lastly to turn to the pollutions of the atmosphere, and to inquire whether there are any aërial scavengers—any plants or animals engaged in the removal of volatile nuisances. That such nuisances exist cannot be doubted. All land-animals throw off from their skins* and from their respiratory apparatus a large amount of refuse, in the state of gases and vapours, probably also of solid matter in minute particles. The decomposing remains alike of plants and animals, together with the liquid and solid excrements of the latter, and, in short, all putrescent matters whatever, pollute the atmosphere, as is proved by the very fact of the evil odours they emit. Many of these impurities are dealt with by inorganic agencies with which we have here no concern. The carbonic acid breathed out by animals—which is, strictly speaking, a gaseous excrement—is removed by plants, which may thus claim a very prominent place among Nature's scavengers. But we have to examine, further, if there is any animal especially qualified to attack those solid impurities which modern research has detected in the air? To this question an answer has lately been returned in the affirmative, and the animal selected for this duty has been the common house-fly! We have no need to

* An exception will probably exist in the case of such species as are covered with hard, corneous, or chitinous layers.

be surprised at such a choice in an age which has produced Kingsley's "Ode to the North-east Wind," and certain attempts to "rehabilitate" the characters of Robespierre and of Henry VIII. It was first asserted that the gambols of the fly in the air of a room were to be compared to the evolutions of the bat or the swallow; that it was, in fact, hawking for food, and that it caught on the wing and swallowed both any suspended particles of putrescent matter and those animal and vegetable germs which are now believed to play an important part in the propagation of zymotic disease. To this the reply is very simple and very conclusive. The mouths of creatures which hawk for food —such as the bat, the swallow, and the goatsucker—are characterised by their wide gape. They are open traps, which are borne rapidly against the intended prey, and at once close upon it. The mouth of the house-fly, on the contrary, is a narrow tube, admirably fitted for pumping or sucking up fluids or semi-fluids, but utterly unsuited for seizing small solid bodies suspended in the air. The evolutions of flies, therefore, are to be compared rather with the flight of the pigeon than with that of the swallow; they are probably undertaken merely as an amusement, and the occasional collisions between two flies do not arise from their both making a simultaneous swoop at the same germ, but are entirely sportive in their character.

Scarcely, however, have the claims of the house-fly as an aërial scavenger been got rid of by this consideration, than they are re-introduced under a modified form. We read in Hardwicke's "Science Gossip" for October, 1875, that a chemist was struck with the fact that flies, after soaring about in the air for a time, alighted, and wiped their feet, wings, and bodies, with very great care. Observing the process under a microscope, he found that a number of minute germs adhered to the body and limbs of the fly, which it thus stripped off, and finally devoured. He next discovered that the flies in well-ventilated rooms were always lean, whilst those in ill-ventilated places were fat and well-nourished. Upon these observations he founds the practical recommendation that housewives should not set poisonous mixtures for flies, but should merely cover up carefully all articles of food, and seek to get rid of the visitors by careful ventilation. This discovery is brought under the public notice as a confirmation of the "pious adage that every thing is of some use."

Now, that flies are in the habit of cleansing their legs, wings, and bodies, is a very old fact. They perform the

operation principally when they have been partaking of their ordinary food, which, as we hoped was known to all the world, consists of something much more substantial than microscopic germs. That among the matter adhering to their surface such germs may occasionally be found, we do not mean to deny. Flies, in all probability, have their parasites, and are known to be liable to disease depending on a fungoid development in their bodies. That they may attempt to free themselves from parasites, and from the spores or germs of this disease, is not at all unlikely; nor can we feel astonished that the filth—living or lifeless—collected from the surface of their bodies is ultimately swallowed. The same thing is, in fact, done by the cat and all other animals which cleanse themselves by licking. But to assert that what the fly thus collects in the operations of its toilet forms an essential part of its diet, upon which the question of its condition depends, is about as rational as to declare that a cat is kept plump by the lickings of its fur. We know the food of the fly: we see it settling upon every article of our diet, and upon a great many things which not even a Chinaman will eat, and, with its protruded haustellum, imbibing their fluid portions; we find it even sucking up the moisture of perspiration from our skins. According to our observation it frequents those places mainly where the good things of this life abound, quite irrespective of their sanitary condition. That, like most other insects, it does not love what are called "draughty" localities,—premises traversed by strong currents of wind,—is quite true. But let a room be perfectly ventilated, without a draught, and stock it with sugar, sweetmeats, and fruits, and you will find the flies cluster thick as leaves in Vallombrosa. On the other hand, we have noticed the absence—or at least the great rarity—of flies in rooms where no articles of human food were ever brought, even though the presence of atmospheric germs in an unusual degree was highly probable. To take one instance:—About eight years ago we became the inhabitant of a quaint and roomy old mansion in the North of England, at a mysteriously moderate rent. We soon found that the atmosphere—not merely of the house, but even of its terraced gardens—was at times unbearably foul, and that ventilating arrangements and disinfectants were alike powerless. The drainage from the cesspools of a group of houses situate on higher ground had soaked down through the shaly soil, and poisoned the ground beneath the foundations. Here, then, was air in which particles*

* It may perhaps be prudent to remind all whom it may concern that "particle" is not synonymous with atom, or even with molecule.

of putrescent matter and germs might be expected to abound, and where flies—according to the theory we are discussing—should have been fat and plentiful. In the kitchen and the dining-room they were found in about their usual amount, but in the room which we had selected for our study, and where no articles of food were ever brought, they were strikingly rare.

We happen, also, to know a chemical laboratory situate in the basement story of a large block of buildings. The sanitary arrangements of the premises are imperfect, and sewage gases often force their way up the sink in spite of traps. Yet flies are here very scarce. There is no draught to banish them, but there is nothing to gratify their well-known "sweet tooth," and consequently they keep away. It may be argued that the flies would be expelled by acid and corrosive vapours.* This, however, is out of the question, as all operations involving the liberation of such products are conducted in an evaporation-niche of the most approved design.

Again, we have every reason to suppose that the air of sewers, of covered manure-vaults, and of dirty neglected cellars, should be pre-eminently rich in floating germs or cells, and in particles of putrescent matter. Yet flies carefully avoid such localities. Indeed, to darken an ordinary apartment—a step certainly not calculated to improve its sanitary condition—is one of the most approved methods of banishing these intruders.

It may further be remarked that flies, when soaring about in a room, do not visit every part of the space as an animal might be expected to do which was collecting food. They hover backwards and forwards, or wheel round in a circular or elliptical orbit in some given spot, often selecting as their centre the bottom of some suspended object, such as a bird-cage, a gaselier, or the like. They rather avoid the corners of the room. All this is exceedingly unlike the movements

* Insects do not, as might be supposed, invariably shun localities where acid gases and vapours are evolved. The writer has seen the woodwork of old and leaky vitriol-chambers, softened by exposure to atmospheric influences, aided by the frequent escape of sulphurous acid, selected by the wasps and hornets of the neighbourhood as an excellent source of fibre for the paper-work of their nests. In a much-decayed beam of this kind were found a large number of *Coccinella 7-punctata*. In dye- and print-works, during the summer and autumn season, moths are frequently found drowned in bowls of acid solutions of tin. Gnats have been perceived dancing merrily, and to all appearance unharmed, in the orange vapours escaping from a shed in which nitrate of iron was being made. On the other hand, a shed, which had been selected for the preparation of solutions of tin, and which had evidently been the home of numerous spiders, displayed, after a few months, merely the tattered remains of old and untenanted cobwebs.

of the bat or the swallow, and as decidedly like the flight of the pigeon, undertaken merely for exercise or amusement.

We have also to point out the very peculiar statement that the flies in well-ventilated rooms are found to be lean. We have yet to learn that flies attach themselves permanently to certain rooms or houses. We see them flying in and out at doors and windows, always, if provisions are not plentiful, ready to wing their way to "fresh fields and pastures new." It must, likewise, not be forgotten that the house-fly is not the only insect given to hover about with no ostensible purpose save amusement. Who has not seen the gnats dancing on a fine evening, and generally in a very analogous manner, taking their centre from the top of some projecting object, such as the summit of a tree, the top of a chimney, or even a gate-post. Are these, too, capturing microscopic cells and germs, either with their mouths or by adhesion to their limbs? If so, they go to work in a very foolish manner. Were they to disperse, instead of collecting in thousands in one small spot of air, their probability of meeting with good sport would be much greater. The cockchaffer, also (*Melolontha maialis* s. *vulgaris*), hovers—or, as the Germans more happily say, "schwärmt"—in hundreds over the tops of trees in fine spring evenings. Is it cell-hunting? Alas! the farmers and gardeners know too well what is its diet!

From all these considerations we feel bound to declare that the claim of the house-fly to be admitted as an aërial scavenger is not made out, and that—regard being had to its known habits as a propagator of disease and carrier of septic matter—the advice to spare or protect it is most injudicious. The pious adage that everything is of some use may, perhaps, be accepted if qualified by the equally true statement that everything is of some detriment, and that the evil in many cases largely overbalances the good. So far as we are then aware, there is no animal engaged in the task of removing aëriform or volatile nuisances, or in clearing the air of minute suspended solids.

There is still a further objection to the view that flies become lean in well-ventilated rooms for lack of floating germs to feed upon. Prof. Tyndall finds that in air kept perfectly motionless all suspended solid matter is totally absent, and that in such air putrefaction does not occur. The more quiescent, therefore, the air of any apartment, the fewer suspended germs, cells, or spores will be present, and the leaner, instead of plumper, will be the flies!

Summing up the facts which we have thus briefly recorded,

it appears that there are among animals three distinct classes or grades of scavengers:—those which bury refuse, whether for the food of themselves and their offspring, or simply to avoid a nuisance; those which destroy offal by devouring it, but do not taint other matters; and, lastly, those which devour putrid matter, but, being at the same time omnivorous and obtrusive, diffuse the germs of putrefaction and disease. Towards these three classes respectively reason demands that we should assume a very different attitude. The scavenging creatures of the first class we should cherish, defend, and seek to multiply. Those of the second, provided they have no especially dangerous properties, like the wolf or the hyæna, we may tolerate, and in scantily peopled and semi-civilised countries we may even protect. Thus, wherever sanitary appliances are imperfect, it is good policy to preserve vultures, carrion crows, &c., by legal enactments. Against the third class, which diffuse septic poisons, and which prey upon sound and useful matters, we must wage an unrelenting and systematic war.

We have observed that not every kind of nuisance finds, in the economy of Nature, some animal expressly adapted for its removal, especially in a perfect manner. There is no insect which buries human ordure, or that of the Carnivora. If, at least, such a process ever takes place it is rare and exceptional. The dissolved impurities of the water are not met, especially when excessive, and the solid impurities of the atmosphere seem equally overlooked. Further, we find one and the same nuisance attacked by scavengers of all the three classes. A dead bird may engage the attention at once of *Necrophori*, *Silphæ*, and blow-flies; or a piece of excrement be visited at once by Geotrupidæ, Brachelytra, and dung-flies. Now, what should we say of an army where part of the troops were equipped with breech-loaders, part with muzzle-loaders, and part with matchlocks, or bows and arrows? What would be our comments if the commanders of the army were most solicitous to keep up the number of the matchlock men, whilst allowing the regiments armed with the breech-loaders to decrease? Or what should we think of a carrier who should employ, between the same two points, goods-trains, stage-waggons, and pack-horses, giving constantly a larger and larger proportion of the traffic to the latter? Yet these two imaginary cases are exactly parallel to what we find in the disposal of offal. Nature's sanitary service does not form a well-organised system in which provision is made for every kind of nuisance, and where every task is committed to that creature only which is capable of

executing it in the most perfect manner. On the contrary, we find important matters overlooked, comparative trifles meeting with a superabundance of attention, and the best sanitary agents elbowed out of the field by imperfect workers who succeed in virtue of their very shortcomings. Such a state of things is obviously incompatible with the old theory which taught that the various species inhabiting any particular country were especially adapted to its climate, soil, &c., and were, like the component parts of some exquisite machine, ordained each for the due discharge of some important function. But if, with the New School, we regard the Fauna and Flora of any country as consisting merely of such species as have hitherto been able to hold their ground in the struggle for existence, and who possibly, but quite incidentally, render to man—or to the world at large—benefits or injuries, all becomes clear and intelligible. Thus a candid consideration of Nature's scavengers supplies us with valuable evidence in favour of the doctrine of Evolution.

There is yet another consideration :—We have seen that there are animal forms depending for subsistence upon dead matter in every possible stage, from the scarcely cold carcase, or the fruit just fallen from its spray, on to *débris* in which scarcely any trace of organised structure can be recognised. Without such matter these animals, with their present habits and as now constituted, could not exist. The *Necrophorus*, as we now find him, implies small dead vertebrate animals, or possibly Mollusca; the Dynastidæ prove the existence of decaying trees, and the Geotrupidæ that of large herbivorous Mammalia. Supposing, therefore, that every animal has some especial and unalterable function for which its structure is a necessary adaptation, the scavengers of Nature cannot have made their appearance on the scene until those animal or vegetable species whose remains or excretions they were ordained to remove had been for some time in existence.* The carrion-feeders would have been in evil plight if created before the animal population of the world had become numerous, and deaths consequently were frequent. Hence, on the same supposition of special creation and of permanence of function, many of the higher animals must not have succeeded, but have preceded in their origin a multitude of lower

* Except we adopt the grotesque hypothesis that the earth at its creation was stocked with decayed wood, leaf-mould, decomposing dung of sorts, and the bodies of dead animals—a supposition for which precedent could be found even at the present day.

forms.* But if we suppose that animals in the course of generations adapt themselves by degrees, alike in habits and structure, to varying conditions, these difficulties disappear. It is very conceivable that animal forms which at one time preyed upon living beings, or upon growing vegetables, may have gradually begun to subsist upon the dead remains of either (as we see some of the most decidedly carnivorous species do to some extent), and may thus have become refuse-eaters in the fullest sense of the word.

III. THE NEWLY-DISCOVERED FORCE.†

By George M. Beard, A.M., M.D., New York.

SOME of the more important facts in regard to the newly-discovered force I have already several times briefly presented in the "New York Tribune," and other journals. In this paper I shall endeavour to systematise our present knowledge of this force, and with as much clearness as the nature of the subject will admit. It is all the more necessary to make this attempt, from the fact that in the letters to the papers, and in the reports of the lecture I gave on the subject, there were certain omissions and errors that were almost inevitable in the presentation of a new and difficult theme, and, furthermore, some of the more recent and important experiments have not yet been made public. I began to experiment with the new force as soon as the announcement of its discovery was made, and since that time have devoted to it many nights, and certain portions of my leisure hours by day. It is proper to state that I have studied the subject independently, suggesting and carrying out my own experiments, especially those of a physiological character, and have repeated the majority of those made by Mr. Edison. It is proper also to state that from the outset Mr. Edison and his assistant, Mr. Bachelor, have in every possible way, and with great enthusiasm and kindness, co-operated with me, freely contributing their apparatus, their time, and their labour. I am under obligations also to Prof. J. E. Smith, for kindly co-operating in some of the earlier experiments made at the establishment of Mr. Chester.

* The same consideration applies with equal force to the Entozoa and to external parasites.

† Communicated by the author.

President Morton, of the Stevens Institute, Hoboken, also generously placed at our disposal, for experiment, the magnificent apparatus of that Institution, and personally aided us in some of the investigations.

Method of Obtaining the Force.

All that is necessary to generate the force is a galvanic current of considerable strength, interrupted by a telegrapher's key, and passing through a small coil of fine wire. In order to capture the force, a piece of iron or cadmium or carbon may be laid across the end of the coil, or within the coil if it be a single coil. To this piece of metal a wire of iron or copper may be attached, and by this conductor the force may be led off to the gas-pipe or any other earth-connection, or to a stove or stove-pipe, or, indeed, to anything that acts as a conductor. Mr. Edison is quite positive that cadmium gathers the force somewhat better than any other metal. I have not seen or made any experiments in the comparative conductibility of different metals.

A battery current of considerable strength is needed, 5, 10, 15, or 20 Bunsen's cells; the number varying with the coil of electro-magnet used, and with the size of the batteries, and with the strength of the solution. It is probable that the force is generated even when fewer cells are used, but not always in sufficient quantity to give a spark, and hence we have no way of studying it, or of demonstrating its existence. The spark has been obtained when but four or even two cells were used. The interruptions may be slow or rapid. When slow interruptions are used, it is found that the spark of the force appears only on the *opening* of the circuit.

The above is probably the simplest method of obtaining the force, and least liable to error, and is therefore better adapted for experiments. Instead of a single coil without a core, there may be double coils with cores, and across the ends of these a piece of iron or cadmium may be placed. Small spools of fine wire seem to be preferable to large spools of coarse wire, and magnets with large cores do not seem to develope the force—at least our experiments with very large magnets thus far have been failures. From the immense magnet in Stevens Institute, Hoboken, we could get nothing. A number of physicists, in different parts of the country, who have attempted to obtain the force from large magnets, or from magnets with large cores, have entirely failed. The very natural supposition that the larger

the magnet the greater the amount of the force, is not sustained by experiment. The inconsistency is more apparent than actual, for in large magnets with large cores the electricity, it may be supposed, is converted into magnetism instead of this force. A certain amount of suddenness of interruption is necessary to the development of the force, and in the large magnet of Stevens Institute a number of seconds—about fifteen, according to our estimate—elapse, after the closing of the circuit, before the magnet reaches its maximum, while on opening the circuit the magnetism is undoubtedly retained in the magnet for some time.

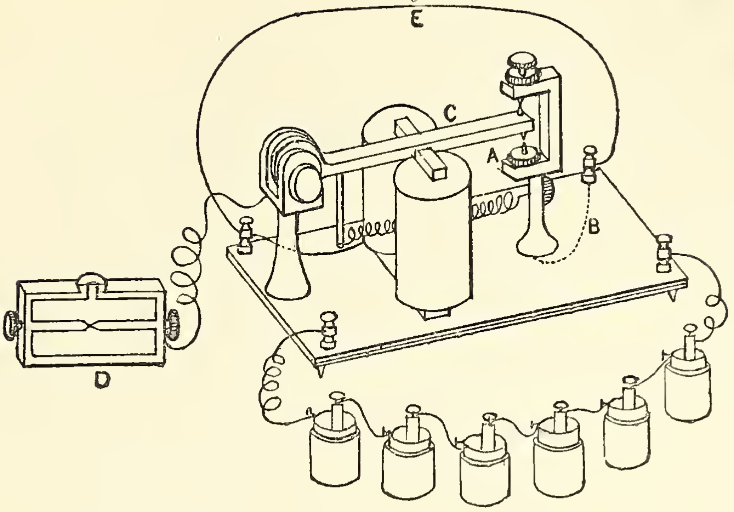
In the small double magnet usually employed by Mr. Edison the yoke is 2 inches long and 3-16ths of an inch thick. The two cores forming the magnet are $1\frac{1}{2}$ inches long, each, and 3-8ths of an inch thick; the coils are composed of twelve layers of No. 23 insulated wire (Fig. 2).

Another method of obtaining the force is by means of self-vibrating electro-magnets of moderate size, the battery power remaining the same as by the previous method. In both methods the principle is the same, a strong interrupted galvanic current flowing through a small coil of moderately fine wire. In ordinary self-vibrating machines, used by physicians, the battery power is insufficient, and the core of the magnet is probably too large.

One of the most convenient methods for obtaining the force is from an ordinary telegraph "sounder" (Fig. 1). On most sounders the extremity of the lever plays between the points of two limiting screws. If the upper screw be insulated from the brass frame which holds it, and is connected to say 10 cells of bichromate of potash or Grove elements, the other end of which is connected to the lever of the sounder, the lever will at once be set in rapid vibration, like the magnetic interruption upon an induction-coil. The spark can be obtained by drawing the edge of a knife lightly across the top of the lever or from the metal at the base. If a wire be connected with one of the binding-screws the force will pass through it to any point, as the gas-pipe or dark box, where we can study it.

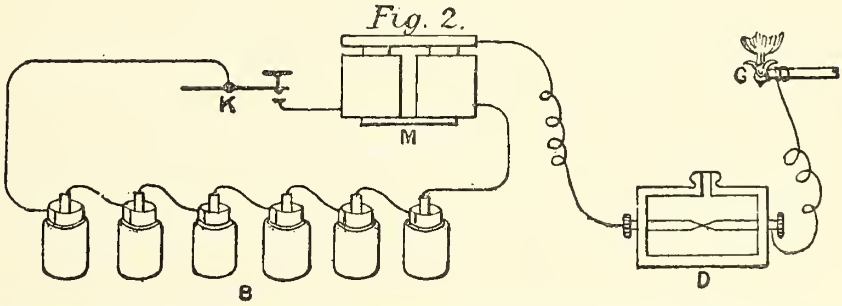
The force may be increased by uniting a number of the sounders connected either with separate batteries or with the same battery. When the force is thus increased, it may be obtained *without metallic contact* with any part of the self-vibrating apparatus: there appears to be a certain area or field in the air through which the force flies away from the electro-magnet, and in this field it may be captured by any

Fig. 1.



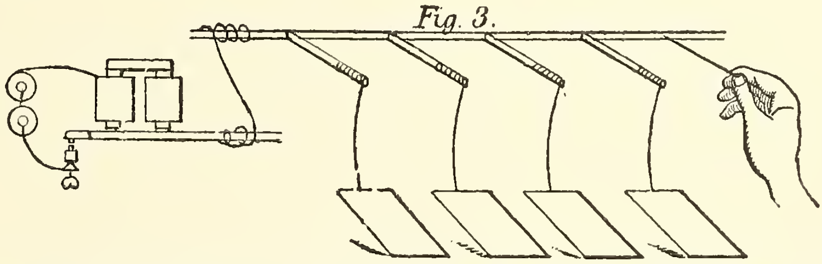
Force obtained from telegraph sounder converted into a self-vibrator.
C, Lever. A, Screw with platinum point, insulated by ring of ivory or rubber. D, Dark box, containing pencil points. E and B, Wires completing the connection.

Fig. 2.



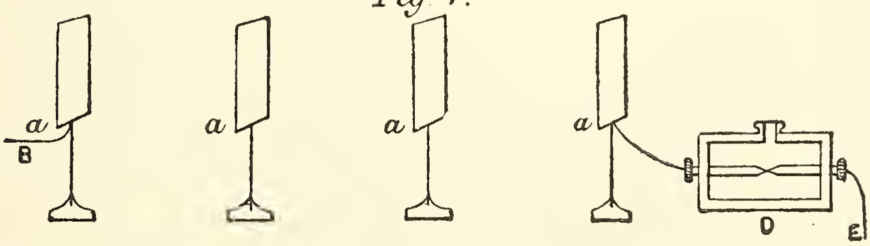
Force obtained from a small magnet, the interruption being made by a key.
M, Small double magnet. K, Key. D, Dark box. G, Gas-pipe or other earth-connection, or any large body of metal.

Fig. 3.



Drawing off the spark after a number of connections with the floor have been made.

Fig 4.



Force passing through the air.
B. Wire conducting-force. *a, a, a, a,* Large pieces of tin-foil suspended on stands. D, Dark box. E, Earth-connection.

metal of considerable surface that is brought near, even when it does not touch the apparatus.

These experiments, together with others to be subsequently described, make it probable that only a very small quantity of the force is captured by the conductors thus far employed, the greater quantity being diffused through the air. Hence I have made the suggestion, which in time will be carried out, to enclose a number of the vibrators with a metallic cone, covered with a layer of paraffin, which, according to my experiments, thoroughly resists this force, and connect the cone with wire similarly insulated.

The force can also be obtained from a Ruhmkorff's coil of moderate size, by drawing a knife across one of the posts near the end of the magnet. It does not seem to make much difference whether or not the outer coil be closed; the force depends wholly on the primary coil. The difficulty of experimenting with Ruhmkorff's coil is that, unless great care be exercised, induction-currents will be obtained. In all these experiments, the battery, the electro-magnet, or coil, may be thoroughly insulated, so as to exclude all possibility of currents of induction over the sides of the cells, and the completion of a circuit through the air. The force can be obtained just as well, however, even when no pains are taken with the insulation. In studying this force the dark box is almost indispensable. A small box of any kind from which the light is excluded, except through a hole at the top, will answer. The pencil points should be carefully sharpened, and should meet in the dark box just beneath the hole in the top.

These pencil points should be made to approach and recede from each other, a distance about equal to the thickness of tissue paper, a number of times, when very weak sparks are to be detected, but when the sparks are strong they may be adjusted to be almost in contact, the fine particles of carbon forming a chain from one pencil to the other. In careful experimenting when the force is passing, or trying to pass, through great resistance, it may be necessary to watch for the spark a number of minutes, varying all the while the adjustment of the pencil points. On the gas-pipe, or stove, or rusty iron of any kind, the spark can be obtained so as to be seen in daylight, provided it be somewhat shaded; on smooth metal it is difficult to get the spark.

Causes of Failure in Attempting to Obtain the Force.

Those who attempt these experiments for the first time may fail to see the spark from any of the following causes:—

1. The battery power may be insufficient.

2. The electro-magnet may be too large, or the core may be too large, or the wire may not be sufficiently fine.

3. Sufficient pains may not have been taken to darken the room. The strong sparks can be seen in full light of gas or day, but feeble sparks can be detected only in moderate darkness. The spark of this force is always comparatively weak.

A micrometer screw is of advantage in making the adjustment of the pencil points in the dark box. In the dark box a spark can be seen and studied when it cannot be readily seen outside. Very small iron wire and rusty tools give the spark better than copper wire or polished metals of any kind, the oxide of iron giving a more brilliant spark.

4. The person making the experiment may have connection with the conductor, and thus draw off a part of the force before it reaches the dark box or other point where it is studied. The body is a good conductor, and error from this source must be constantly guarded against.

Physical Experiments.

Out of a very large number of other experiments, I may mention the following :—I stuck a penknife in a large block of paraffin, and connected it with metal conducting the force, as a gas-pipe or wire, drawing the blade lightly over the conductor. No sparks appeared. When a long file was substituted for the knife, sparks were abundant, and were kept up as long as the connection was made. In these experiments the force appears to pass into the metal, and thence is diffused into the air. I suspended by long pieces of silk rolls of wire of various sizes, and allowed them to strike against the connection. With small coils sparks rarely appeared; with the larger coils they were abundant. It would seem, therefore, that a certain size is necessary in the conductor in order to get the sparks. A short bit of wire wound round a glass rod, and held against the conductor, would not get any spark; but take the same bit of wire connected at the other end with a spool of wire, or any large or long metallic surface, and the spark at once appears. A large surface of metal seems to attract the force better than a small surface. For this reason it is an advantage to connect the distal pencil point in the end of the dark box with the gas-pipe or other large metallic conductor: it is not, however, necessary to do so, for the spark will appear when the lead pencil is isolated. At one time I led the wire

through a large vessel filled with water, and pieces of iron and bars of iron of various sizes were placed across its track and rested upon the wire, and the wire was wound round an iron press, and yet at the end the spark appeared. Mr. Edison took the wire out of doors, ran it along the ground and in a ditch on a rainy night, and brought it up-stairs several rods from the battery, and the spark was seen by him, by his assistant, and by myself in the dark box above described, but it was not constant, and required a nice adjustment of the carbon points to bring it out. The force, therefore, does not readily leave the metallic conductor, even when in contact with the earth or passing through water, and the spark is seen when the end of the wire conducting the force is at a long distance from the battery.

Physiological Effects.

1. The force is conducted by the human body. This was proved by taking hold of the conductor—a wire, iron bar, or gas-pipe—that was in connection with the apparatus, evolving the force by one hand, and with the other touching the blade of a knife to a stove or block of metal: sparks appeared, though somewhat smaller than when the force did not traverse the body. In some of these experiments, which were tried on several individuals, the body was insulated by a large block of paraffin 6 inches thick. When the distance for the force to travel through the body was reduced one-half—by making the connection at the back of the neck or in the mouth—a somewhat larger spark was produced than when the whole resistance of the body from hand to hand was included. It was clear, therefore, that the body conducted the force, though not so well as metals. A person standing on an insulator, with the conductor in hand, does not, on dropping the conductor, give any evidence of being charged; he can give no sparks to any other person or to any metal. I have also found it impossible to charge metals.

2. The force in passing through the body produces no demonstrable physiological effects. While we have the evidence of the sparks that the force is traversing the body, yet, wherever directed, it causes no sensation, not even on the tip of the tongue, no muscular contraction anywhere, no tremor, no erection of the hair, no flashes of light, no sour taste, no dizziness—in short, none of the usual physiological reactions of the different forms of electricity. Mr. Edison had supposed that in his own case contraction of the muscles

of the tongue was produced when he applied the tip to the conductor, and his head did really move up and down as though the muscles were affected, but upon my breaking the connection unknown to him his tongue kept moving as before, synchronously with the respiration. It was a case of mind acting on body ; he expected some effect, and unconsciously produced it himself. Mr. Edison and two of his *collaborateurs* were taken sick in various ways one night, and it was supposed that the illnesses were caused by the force, but in this also they were probably mistaken ; mind acting on body or coincidences may account for their symptoms. It is certain that I have experimented many hours by day and by night with this force, and a considerable portion of the time it was passing through me or into me, and I was not unfavourably affected, nor were any of those employed in the establishment, including Mr. Edison and the others who fancied their illness was caused by it. What effect the force evolved from a much more powerful apparatus, and passed through or into the body for a long time, may have, primarily or secondarily, I cannot say. Some who tested the matter thought that a very slight tingling sensation was experienced on the tongue, but closer examination did not confirm this.

3. The force, when generated in sufficient amount, causes the galvanoscopic frog to contract. It is well known that the irritability of frogs varies with the season of the year, and also with other conditions ; hence the galvanoscopic frog cannot be an absolute standard or measure for electricity. For this reason, in all these experiments, the irritability of the frog was tested by a galvanic current passing through definite resistances. We tested the frog used in these experiments, and found it so sensitive that one electropoion cell, placed in a circuit having a resistance of 400,000 ohms, or nearly 35,000 miles of telegraph wire, caused contraction, and yet it did not contract when this force was passed through it. That the force in these experiments passed through the frog (which was insulated) was proved by the spark that appeared at the distal end. In this experiment, the result of which was most remarkable and unexpected, all conceivable elements of error seemed to be excluded. Subsequently—with a different apparatus, a Ruhmkorff's coil—a contraction of the muscles of the frog was obtained. The experiment was made at the establishment of Mr. Chester, and repeated in Newark.

In a subsequent series of experiments, made with Mr. Edison, contractions were obtained in the frog's leg, although

the apparatus was most thoroughly insulated. As galvanoscopic frogs are susceptible to mechanical irritation, it was suggested that possibly the vibrations, from the apparatus communicated through the wire, caused the contractions, and on using the key (Fig. 2) instead of the self-vibrator the force caused no contractions. Returning to the self-vibrator (Fig. 1) contractions appeared. When the wire connecting the apparatus with the frog was shortened, the contractions increased in vigour. That the frog was susceptible to vibrations was shown by striking a very large tuning-fork and touching it to the sciatic nerve, and sometimes contractions appeared when the vibrating fork did not touch the nerve, but was held at a distance of $\frac{1}{2}$ or $\frac{3}{4}$ of an inch from it. It was shown, however, by experiments on pith balls, that electricity is generated by a vibrating tuning-fork, and this electricity—which is probably the result of the impact of the steel against the air—may possibly have caused the contractions in the frog. On testing this frog by the galvanic current it was found that one electro-poion cell, after going through a resistance of over 1,000,000 ohms, or about 75,000 miles of telegraph wire, easily caused contraction.

In later experiments, however, made with Prof. Smith, I failed to cause any movement of the frog by mechanical vibrations alone, even when the nerve was held close to the self-vibrator; but when the force was allowed to pass through the nerve and muscles of the frog it contracted. When the key was used instead of the self-vibrator the frog did not move, even on the opening of the circuit. When the force coming from the self-vibrator was passed through several inches of water, so as to eliminate the error of mechanical vibrations, the muscles of the frog contracted. It is probable, therefore, that when developed rapidly and in large amount, as in the self-vibrator, this force causes contraction in the galvanoscopic frog.

The presence of electricity, in its different forms, is determined by the electroscope, the Leyden jar, the galvanometer, the electrometer; electrolysis, or electro-chemical decomposition, by physiological effects, and by light, heat, or ozone produced. This force, as thus far studied, does not deflect the leaves of the electroscope, nor charge the Leyden jar, nor move the needle of Thomson's delicate reflecting galvanometer or electrometer, nor decompose iodide of potassium, nor produce any demonstrable physiological effects on the nerves of motion or sensation, or speech, or on the muscles or other tissues; nor does it under all conditions

always affect even the galvanoscopic frog, the most delicate of all tests of electricity.

On the different forms of calorimeter it has not been fully tried. I have convinced myself that, like electricity, it is resisted somewhat by platinum wire, and it is possible that in passing through platinum a portion of it may be converted into heat, as is the case with electricity; and if so a delicate thermometer, the bulb of which is surrounded by platinum wire through which the force is passing, would be affected. The heating power might be tested by the thermo-electric pile and galvanometer, or by the differential electro-calorimeter: experiments of this kind, however, whatever the results, would do but little toward solving the problem of the nature of the force. The odour of ozone, that is observed from the spark of dynamic electricity, I have not been able to obtain from this force.

It appears, then, that the light, as seen in the spark, and the contraction of the frog, are the only evidences we have of the presence of this force in any conductor. It is changed into light, as is electricity, in passing from one metallic conductor to another. This spark has yet to be exhaustively studied by the microscope and spectroscope. It is possible that it may affect some of the chemical substances chemically or thermically.

Non-Polarity of the Force.

The apparent non-polarity of the force appears in all its phenomena. Although, like light and heat, it may be capable of polarisation, yet, practically in the ordinary phenomena it is apolar, like those two forces, and as such it may be regarded. The idea of a circuit is not suggested by anything that is done with the force. We draw it off from the conductor as we draw off water from a spout, gas from a pipe, or heat from a stove. It has no tendency like statical electricity to distribute itself through the earth any more than any other conductor. When a direct passage to the earth or the walls of the room is established, by a gas-pipe, for example, it can still be drawn off from any branches of the pipe between the apparatus and the floor. It is captured by any good conductor, as metals or the human body, that is brought near to or in contact with a metal already conducting it.

With all the known forms of electricity, however they may differ in their special phenomena, the fact of polarity is inseparably associated; and it is by virtue of its polarity that electricity accomplishes work. Take away from the

different forms of electricity their polarity, and you take all their practical usefulness in telegraphy, in electroplating, in signalling, in medicine, or in surgery.

Conceivable Sources of Error.

The presumption against the actual demonstration of the existence of a new force is very great, and can only be overcome by evidence of an overwhelming character. The experiments must be repeated with substantially similar results, at various times, and by different expert observers. When, however, the phenomena are admitted, and when it is admitted that they cannot be explained by laws of electricity as known to experts in that branch of science, the burden of proof is shifted, and the presumption is against the claim that these phenomena represent some known phase of the electrical force.

Mr. Edison and myself, and a number of physicists, who have thus far studied the subject, have made earnest and sustained efforts to prove that some known form of electricity would account for all these phenomena. Four theories of electricity have occurred to us, and by all of them these phenomena have been tested.

First. Creeping Electricity, which passes over the sides of the cells, and completes the circuit through the earth and air. This is the theory that would naturally occur to any physicist on first examining the phenomena. Besides the general fact that this force does not respond to the ordinary tests of induced electricity, this theory is met by two facts, either of which seems to be sufficient to overthrow it.

1. The phenomena of the force appear just as well when the cells of the battery and the entire apparatus are most thoroughly insulated. In one case the insulation was so complete that when the entire apparatus on the insulating stands were charged by statical electricity, the charge was retained for a long time, so that sparks could be taken from it; and yet the force appeared fully as strong as when there was no insulation.

2. In order to complete the circuit, it would be necessary for the induced electricity to traverse immense distances in the air, and at the same time have sufficient strength to give a decided spark. This is inconceivable; and, further, it is shown by direct experiment that this force, though it can go through the air when the surfaces at the ends of the conductors are sufficiently large, yet only for a few inches or a few feet at most, and when the ends of the conductors

are small wires the force will not pass—any long interval, at least—in sufficient quantity to produce a spark on the other side.

Secondly. The Extra Current under Ordinary Conditions of a Circuit.—It has been suggested that the force might be the extra induced current; but that current, as is well known, obeys all the laws of the other forms of induced electricity, produces decided chemical and physiological effects, causes a deflection in the needle of the galvanometer, everywhere gives constant evidence of polarity, and, so far as is known, cannot exist without a circuit. The spark of this force does not scintillate as much as the spark of the extra current. Nothing is easier than to prove the presence of the extra current; whatever this force may be, it cannot be that current in ordinary circuit.

Thirdly. Statical Electricity of High Tension.—High tension statical electricity, such as is obtained from statical machines, gives a jumping spark; its physiological effects are very powerful, and even dangerous; it can charge bodies and things; and when connected with the earth by a good earth-connection it at once disappears. The spark of this force is scintillating, not jumping, requiring light contact to obtain it. Examined under the microscope, even, it does not appear to jump any more than the spark of dynamical electricity. Moreover, this force has no demonstrable physiological effects, cannot charge persons or things, and when connected with the earth or floor by the best possible connection it can still be drawn off from the conductor.

Fourthly. Statical Electricity of Low Tension.—Low tension statical electricity obeys the laws of statical electricity of high tension in this respect—that it totally disappears when connected with the earth or with the walls of the room which are supposed to become polarised by it. Again, low tension statical electricity affects the electroscope and electrometer, and ought to charge the Leyden jar. Thomson's quadrant electrometer, which this force thus far has not affected, is a very delicate test for low tension statical electricity.

Mr. Edison has found that low tension electricity, such as is obtained from the free poles of a single galvanic cell, does not behave at all after the manner of this force. When the tension is increased up to sixty-five cells, and is further increased by employing a condenser composed of numerous sheets of tin-foil separated by paper dipped in paraffin, and having a capacity of 34 microfarads, yet when connected with the earth the electricity at once and entirely

disappeared, and could not, like this force, be drawn off from the sides of the conductor.

Theories of the Force.

The theory which Mr. Edison favours is that this spark indicates a radically new force, to which he has given the provisional name "Etheric," from its tendency to diffuse itself in various directions through matter. This theory would regard this force as distinct from any form of electricity, as light or heat, and would indeed, bring it nearer to heat than to electricity, or would make it a kind of intermediate between those two forces. In the ultimate analysis there is probably but one force, of which light, heat, and the different kinds of electricity are modifications—modes of motion, differing in the nature of their vibrations, correlated to and capable of being transformed into each other. The old-fashioned fluid theories of electricity, although they still hold ground in school and college text-books, have been long since abandoned by physicists, and in their place the theory that it is a mode of motion of the ether, and of other matter through which it circulates, is more in harmony with recent science. Already several varieties of electricity have been discovered as follows:—

Frictional or statical electricity.

Dynamical or current electricity (including galvanic, induced, and thermo electricity).*

Induced electricity is farther subdivided into primary, extra, secondary, and tertiary currents, which differ from each other in quantity, tension, and in physiological effects. The tendency is for these different forms of electricity to approach to and actually run into each other.

An electro-magnetic apparatus is a reservoir of many forms of force—galvanic and induced electricity of various orders, magnetism, statical electricity, light, and heat. A source so rich in forces might give us at least one more; it is possible that one more has been here discovered. When a strong galvanic current, connected so as to flow through a coil of wire, is closed, a magnetic field is evolved, in and near the coil and at some distance from it, the electricity being converted into magnetism; when the circuit is open, the electricity, it has been supposed, is converted into heat; it is probable that some of it is converted into this new force.

* The statement recently made that thermo-electricity differs radically from any other form of electricity is incorrect. Thermo-electricity obeys the laws of dynamical electricity, however obtained.

In suggesting the theory that this force might be allied to electricity by supposing it to return, after the manner of the shuttle, to the source whence it is generated, I did not by any means commit myself to it; on the contrary, when all the known facts and phenomena that relate to the subject are carefully balanced, I find it as yet impossible to disprove the theory that *this is a radiant force, somewhere between light and heat on the one hand and magnetism and electricity on the other, with some of the features of all these forces.* But this claim is stupendous.

Experiments of the following kind are suggestive to enquirers in this department of research. When the wire conducting the force from the battery to the dark box is divided in the air, and the ends are separated even a sixteenth of an inch, no spark appears in the dark box. Lay these ends of the wire on a semi-conductor, as wood, and the force will pass even when they are separated a moderate distance. Place small pieces of tin-foil about these ends as they are again suspended in the air, and the force now passes an inch or perhaps several inches through the air. Place pieces of tin-foil of large surface about these ends, and the force will pass a longer distance. Make the surfaces of tin-foil larger still, until they are a foot square or more, and the force will travel several feet through the air.

Prepare three large pieces of tin-foil; place one piece at each end of the divided wire suspended as before, and the other piece about equal distance between them; and still the spark may be seen faintly, though irregularly, in the dark box. The force must go by induction or radiation from the piece of tin-foil to the middle piece, which acts as a kind of resting-place, and thence to the piece at the other end of the wire. The spark has been obtained, though with difficulty, and only after very nice adjustment of the pencil-points in the dark box after having passed through four pieces of tin-foil, the distance from the first to the last being *eight feet* (Fig. 4). The highest tension statical electricity, as generated by Holtz's machine, could not do this except by induction, and withal would require insulation.

When a number of Leyden jars are substituted for the pieces of tin-foil, the result is the same; but Leyden jars are insulated. In these experiments insulation is not required, as is shown by the following experiments:—

A large surface of tin-foil (6×6 or 12×12 ins.) was connected with one end of the divided wire and laid on a table. Over this were placed broad pieces of rubber, glass, or paraffin, and on the top of them was placed a similar piece

of tin-foil connected with the other end of the divided wire, through which the force was conducted to the dark box. In this way I proved that the force could pass through $2\frac{1}{4}$ inches dry wood, two plates of glass each $\frac{1}{4}$ of an inch in thickness, $\frac{1}{4}$ of an inch of hard rubber, $\frac{1}{4}$ of an inch of solid paraffin, and 5 layers of paraffin paper. When the surfaces at the end of the wire were reduced in size, or when the tin-foil at one end was removed, the force passed less easily. When the tin-foil at both ends was removed, and only a few inches of fine wire constituted the surface, the force passed through only thinner resistances, and when *only the terminals* of the wire were applied to the surface of the resisting body the force would not pass at all, or but a very slight distance. The force passed through 20 inches of water in a small tube, and was apparently but little diminished even when the surface at the terminals was but the diameter of a small wire.

When a number of "sounders" are in action near together, and connected with the same or different batteries, the force can be captured by a piece of wire a few inches long, connected with the dark box, and held at a distance of several inches from any of the sounders. This is, however, no exception to the law that the force passes resistance by surfaces, for the sounders here constitute one surface and the few inches of wire the other, making a condenser or Leyden jar.

Some of the early experiments with this force gave erroneous or unsatisfactory results, because it was not known or suspected that with surfaces at the terminals it would pass through bad conductors even when the apparatus was not insulated.

In experimental researches we may learn oftentimes more from our blunders than from our successes. In studying the passage of this force through glass, the mistake was made of overlooking the conductibility of the air and the human body. On a long glass rod, held by a stand, were wound the terminals of the divided wire through which the force was passing. Before the rod stood one of the experimenters, placed his hands on the ends of the wires and pushed them an inch apart, and the spark appeared in the dark box. He pushed them several inches apart, still appeared the spark; yet farther, the entire length of the glass rod, 2 feet or more, yet the spark—though fainter—was easily seen. The inference, which for a number of days was unchallenged, was that the force passed through the long glass rod, though somewhat resisted by it. This inference was wrong. We

afterwards found out, by a combination of accident with closer observation and a better knowledge of the conductibility of the air and the body, that the force in this experiment was all the while passing not through the glass rod at all, but through the body of the person adjusting the wires, or through the air from between the surface of his hand and the terminal; with one hand on one terminal and the other brought within a foot or more from the other terminal, the spark appeared.

Mr. Edison suspects, and he may be right, that in certain barometric and hydroscopic states of the atmosphere the force may pass through long rods of glass; but in the many experiments that I have made since the discovery of this error here noted, I have never been able to see the spark when any considerable length of glass or rubber or shellac was interposed between the terminals, excepting when there was at one or both of the terminals a large conducting surface.

Phenomena of this kind suggest magnetism more than inductive or dynamical electricity, but this force does not respond to the test of magnetism, the power to attract iron, and moreover exhibits phenomena that do not belong to magnetism. This force is attracted by iron and other metal as conductors, but it does not appear to attract iron.

The points which favour the theory that this force, whether electrical or radiant, is yet something new to science, may be thus recapitulated:—

1. It does not respond to any of the physical tests of electricity, except the spark.
2. It produces no perceptible or demonstrable physiological effects like electricity, save on the frog.
3. It gives no evidence, in any of its phenomena, of polarity.
4. It passes through the air and other resistances by large surface at the terminals, even when the apparatus is not insulated.
5. When connected with the earth or walls of the room it can yet be drawn off from the conductor.

Any known form of electricity giving a spark like the spark of this force would respond to some of the physical tests of electricity; would produce readily perceptible physiological effects; and would in its phenomena suggest polarity, even if rapidly reversed.

Again, the four facts regarded by me as favouring the theory that this force is allied to electricity are, when severely analysed, not so convincing as they might at first appear.

The spark of this force resembles the scintillating spark of dynamic electricity, so also do sparks produced by combustion. The velocity of this force is great, but so is that of light. This force is best conducted by metals, but so also is heat.

This force is resisted by non-conductors, but heat is similarly resisted, and both to a less degree than electricity. If it be as I have suggested, a form of electricity which is so rapidly reversed as to be practically depolarised, it would yet be electricity under very different conditions from those under which we are wont to consider it, and would be really a new force. The more I experiment in this department, and the more closely I reflect on the results of experiments, the farther I seem to be driven from the electrical toward the radiant theory of this force : in either case there would appear to be no ready escape from the acceptance of the conclusion that we have here something radically different from what has before been observed by science.

The relation of this force to the other forces may be thus represented :—

Light, Heat, . . New Force, ... Electricity, Magnetism.

The above would represent Mr. Edison's theory of a radiant force, nearer to light and heat than to electricity or magnetism.

The theory I have suggested would ally the force to electricity or magnetism more than to light and heat, as follows :—

Light, Heat, ... New Force, Electricity, Magnetism.

Presumption against the Validity of the Claim.

But it is yet too early to accept either theory. Although in the abstract there is no reason why a new force, or several new forces, might not exist in nature, the phenomena of which should be revealed by chance or otherwise, and which are now unknown and unsuspected, because we have no way of knowing the conditions necessary to produce them ; yet practically, and in the concrete, the presumption against the validity of any claim to the discovery of a new force is in the first instance enormous, and can only be overcome by an enormous amount of expert evidence. Science it is said is sceptical ; it ought to be sceptical. The sources of error in studying phenomena believed to be new are so numerous that the statements of the first experimenters cannot be

accepted, and ought not to be accepted, until they have been confirmed again and again by competent experts.

Previous Attempts to find a New Force.

During the past forty years strenuous efforts to discover a new force have been made by scientific men all over the world. Mr. Edison himself, so he informs me, has made a series of elaborate experiments in this direction. In several instances men have fancied that success had crowned their efforts; hence have arisen the delusions of odic force, started by Reichenbach, with which this discovery of Mr. Edison has been absurdly confounded, and of mind-reading, or the power of conveying thought from one brain to another without the aid of the ordinary senses, as recently announced by some of the scientific professors of America.

For three reasons all these claims have been rejected by the scientific world. First, those who made them were not authorities on physiology, to which department their alleged forces belonged. Secondly, the experiments supposed to prove the existence of these forces, if we may accept the accounts given by the authors, were complicated with numerous and fatal elements of error, chiefly coincidences, mind acting on body, and trickery, any one of which would destroy the value of any scientific experiment. Thirdly, the alleged results have in no instance been satisfactorily confirmed by any expert in experimental physiology.

The claims of this newly-discovered force relate both to physics and physiology, and it is by physicists and physiologists, who are trained to habits of experimental research in their respective departments, that it should be studied.

The question whether there is in the human body any form or manifestation of force differing radically from those already known, I had already thoroughly investigated, and had reached long ago the decided conclusion that there was no evidence or suggestion of evidence of the existence of such a force.

Relation of Accidents to Discovery.

Into this and previous discoveries of a similar character the element of chance has largely entered; that is, fortunate accidents have occurred to experts capable of appreciating their meaning.

It was by accident that Galvani observed the twitching of

the muscles of the frog, but for twenty years prior to that observation he had been studying the phenomena of electricity: it was by accident that Oersted discovered that a needle in the neighbourhood of a coil in which a galvanic current was circulating was put at right angles to the coil, but for fifteen years he had been seeking to accomplish this object. The spark of this new force coming from the core of a small magnet was accidentally discovered by Mr. Edison on the night of November 22nd, 1875; it has been often seen before by practical electricians and by others, and it had been assumed to be inductive electricity; it had been seen before by Mr. Edison, and had been unnoticed, but here, as everywhere, it was proved that the eye sees what it brings the means of seeing,—that it is not the eye, but the brain behind the eye that sees; for this time the spark was observed by one made specially alert by long practice in original experimental research in this department of enquiry.

If these experiments shall be so far confirmed as to become accepted by those physicists and physiologists who are competent to deal with questions of this nature, the discovery must assume great importance. It is forty-five years since any force has been introduced into science—the discovery of induced electricity by Faraday, dating from the year 1830. It is eighty-nine years since the discovery of galvanism; seventy-six years since the invention of the voltaic pile; one hundred and twenty-three years since Franklin, by his kite experiment, showed the identity of electricity and lightning; one hundred and thirty years since the discovery of the Leyden jar; and two hundred and seventy-five years since the publication of Dr. Gilbert's "Tractatus de Magnete," that marks the birth of the science of electricity.

Questions of Priority.

Close on the heels of every alleged discovery follow questions of priority. Already it has been claimed that the experiments of Riess, the great German authority on statical electricity, in obtaining weak sparks from the Leyden jar and Holtz machine, had really anticipated the experiments recorded in this paper. It is just to say that no such claim has been made or would be made by Riess himself, nor indeed by any one familiar with both series of experiments. The weak sparks of Riess were sparks of statical electricity; they exhibited polarity, and in other respects

conformed to the laws of that form of electricity; he did not perform, and did not attempt to perform, with these sparks any of the physical or physiological experiments here recorded, nor did he claim to have discovered any new force or any new form of electricity.

The coincidence in time between the publication of the abstract of Riess's experiments and the announcement of the discovery of the new force is admitted, and if the series of experiments were identical there would perhaps be a just reason for raising the question of priority.*

Practical Value of the Force.

The practical value of this force it is yet too early to discuss. There is no evidence, as yet, that the spark can be seen after the force has traversed very long distances, beyond a quarter or half a mile. It is conceivable that it may yet be generated in sufficient quantity to send a spark through the outer wires of the Atlantic cable, and thus it may expedite ocean telegraphy; but there is as yet no proof that it can be utilised in that way or do any important work whatsoever.

Ii is entirely possible that it may produce important physiological effects, although such effects have not yet been demonstrated. If it be a radiant force, analogous to light and heat, it may, like those forces, affect the system radically, without giving rise to any sensation. Light is important, even indispensable for healthy life, yet its influence is never immediately or directly felt, save on the eye; and heat of a moderate intensity the body may receive unconsciously.

For the present, then, the question in regard to this force is primarily one of science, and it is to be studied by scientific more than by practical men, belonging rather to physicists and physiologists than to telegraphic engineers or physicians.

Time and many experiments may be needed before we shall be enabled to so increase and control this force as to make it supersede or supplement any of the ordinary uses of electricity. The history of electricity shows that long intervals often occur between scientific discoveries and their practical applications. Thus galvanism was discovered thirteen years before the invention of the voltaic pile; a number of years more passed before the pile was utilised in

* The paper of Riess was published originally in "Poggendorff's Annalen," and the abstract referred to appeared in the "Electrical News," Nov. 15, 1875.

1807. Induction, discovered by Faraday in 1830, was not utilised in telegraphy for a long time, and was not formally introduced into medicine by Duchenne until after the lapse of seventeen years.

But even though this force shall never be used in the arts, or applied to any commercial purpose whatsoever, it must be, if its existence is established, of the highest scientific interest.

Every new fact or thought is, of itself, a positive addition to the world's wealth, and, however isolated at first, must in time affect both the past and the future, illumining the one and guiding the other.

Whatever the conclusion of physicists in regard to these alleged phenomena may ultimately be, the discussion of the subject cannot fail to be of interest and of value to the student of electrology, and for the reason mainly that it compels a re-investigation of the laws and phenomena of the different forms of electricity, in the light of the most recent theories of that force. A knowledge of electro-physics is indispensable to a thorough knowledge of electro-physiology or electro-therapeutics, medical or surgical: the value of many experiments in physiology, those of Ferrier for example, are seriously impaired for want of such knowledge. If it should be proved to the unanimous satisfaction of competent judges that these phenomena, so far as they are genuine, simply represent some well-known form of electricity, the labour and the time given to the subject will yet have been well spent.

For my own part, while I have all along inclined to the conservative view, that this spark indicates some form of electricity, and by that theory was first drawn to the study of it, I have yet been unable to prove this, or to obtain such proof from any source. Among the physicists who have given thought to the subject, and with whom I have conversed or corresponded, there is a wide diversity of opinion. Some men of ability and reputation have found great difficulty in getting the spark at all; others of equal ability and reputation have obtained the spark, and have satisfied themselves that it is merely the spark of the extra current of induction; others still are positive that it is statical electricity of low tension. No one, however, so far as I know, has published any facts that explain these phenomena by the unanimously admitted laws of any form of electricity; and no one, so far as I know, has yet repeated all the experiments here recorded.

Thus far those who are familiar with the *technique* of

electro-physics, and who have the sounders and small magnets at hand, as telegraphers and telegraphic engineers, have been more successful in obtaining the spark, and are more nearly unanimous in the opinion that it represents something that cannot be explained by known laws of electricity, than professors of theoretical chemistry.

In conclusion, I may say that although I originally suggested the hypothesis that this force might be electricity so rapidly reversed as to be unable to respond to the usual tests, and therefore practically depolarised, and without a circuit in the ordinary sense of that term, and have kept that theory in mind in all my investigations, and hence would not unnaturally be pleased to have it proved to be the true solution of these phenomena, yet I cannot blind myself to these serious difficulties in the way of its acceptance :—

1. This force, when key is used (Fig. 2), and the interruptions are made slowly and carefully, appears only on the *opening* of the circuit. Where, then, is the opportunity for rapid reversals? Besides, induced electricity, even when very rapidly reversed, affects delicate galvanometers.

2. The physiological experiments are not fully accounted for by this theory. Any form of rapidly reversed electricity in an ordinary circuit, giving a spark like the spark of this force, is felt on the tongue and lips, and in the inner corner of the eye, and indeed on less sensitive parts of the body.

The galvanoscopic frog ought to contract even when the key is used with slow interruptions.

3. The passing of this force through great resistances of air, and solid non-conductors when large surfaces are at the terminals (Fig. 4). In these experiments these resistances may be supposed to act as dielectrics, and the whole arrangement may be regarded as analogous to a Leyden jar, or the condenser of a Ruhmkorff coil; but the extra current, as it passes through a condenser, is in a circuit: here there is no demonstrable circuit, and, furthermore, the dielectrics in the condenser are trifling in extent compared with the extent of air and thickness of resistances in these experiments. According to this hypothesis of rapidly reversed currents, induced electricity, when it reaches the large terminal, is changed into statical electricity, and at the distal large terminal is changed back into induced electricity, and through all these varying conditions the electricity is going backwards and forwards so rapidly as to be unable to respond to the usual tests. Is this demonstrable?

These objections may not be fatal to this working

hypothesis of rapidly reversed electricity, but they must be met and fully considered by those who are inclined to favour it. It is the more important to insist on these difficulties, because a number of observers who have repeated some of these experiments, and admit the phenomena or some of them, are taking instant refuge in this theory as though that solved the whole mystery.*

Both theories, that of a radiant force and that of rapidly reversed electricity, are radically new suggestions, and neither of them can be, or will be, or ought to be accepted, except under the pressure of long-accumulating evidence.

Meanwhile it should be borne constantly in mind as a principle of evidence that for those who admit these phenomena, and who assert that they indicate some known phase of electricity or magnetism, the burden of proof in that issue rests to make good the claim by positive proofs. Until such proofs are obtained the scientific mind can maintain a position of neutrality, or can consider the relative value of the two theories here suggested. Either course would be consistent with the scientific spirit. The theory of rapidly reversed electricity apparently accounts for but a part of the phenomena, but it has this advantage over the radiant theory—that there is less presumption against it. If we could suppose a phase of statical electricity of low tension and considerable quantity, which, for some reason,

* Prof. Houston, of Philadelphia, among others, has repeated some of these physical experiments, and has adopted in full, after but a partial study of the subject, the hypothesis of rapidly reversed electricity as suggested in my letter to the "Tribune" of December 9th, and further claims priority of discovery, because he observed the spark of this when experimenting with a Ruhmkorff coil four years ago. To this claim, if it be seriously entertained, the obvious reply is that thousands of persons, probably, had seen this spark before it was *discovered* by Mr. Edison: it had been seen by Prof. Nipher, who supposed, and still supposes, it is the spark of the extra current; it has been seen by my friend, Prof. J. E. Smith, who assumed, as he tells me, without examination, that it was inductive electricity breaking through bad insulation; it had been seen, as has been stated, by Mr. Edison many times before he thought it worthy of study; it was undoubtedly seen by Prof. Houston, who, like so many others, failed to even suspect its meaning, and thus missed an important discovery. The honour of a scientific discovery belongs not to him who first sees a thing, but to him who first sees it with expert eyes; not to him, even, who drops an original suggestion, but to him who first makes that suggestion fruitful of results. If to see with the eyes a phenomenon is to discover the law of which that phenomenon is a part, then every school-boy who before the time of Newton ever saw an apple fall was a discoverer of the law of gravitation. Prof. Houston's account of his repetition of some of these experiments was published in the "Journal of the Franklin Institute" for January, 1876; and the article in which he incidentally mentions sparks that he now rightly recognises as the spark of this force was published in the same Journal, June, 1871.

is unable to respond to most of the electrical tests, these phenomena might perhaps be explained; but such a supposition really begs all the questions at issue.

IV. BIOLOGICAL CONTROVERSY AND ITS LAWS.*

MANY edifying commonplaces might doubtless be written on the intellectual fermentation, if it may not rather be called confusion, of the age. Nor can it be denied that tendencies supposed to have been long ago slain and sepulchred have risen again, and are asserting themselves with a hardihood which our fathers would have deemed impossible. When we find a scientific work—at any rate a work written by an eminent scientific man, and devoted to the discussion of scientific questions—formally dedicated to a dignitary of the Catholic Church as a vindicator of the rights of conscience (!), we may well ask, not jeeringly but sadly, "What is truth?" We have witnessed of late brilliant progress in various departments of science; but we have also seen attacks made upon the very foundations of science. These onslaughts are increasing in frequency and in boldness. Metaphysicians and ecclesiastics are calling in question the inductive method, impugning the independence of Science, and seeking to re-assert over her the authority of "the Church." The battles of the sixteenth century seem about to be repeated. And some, who might claim to be the heirs of Galileo, think it no ignominy to wear the livery of Bellarmine and Caccini.

When we first opened the book which has suggested our present article we fully expected to find an intellectual treat of the highest order: its subject is one on which a most valuable work might well be written, and few living men indeed are better qualified to undertake such a task than is Mr. Mivart. Anti-Darwinian polemics we awaited, but such criticism, if conducted on legitimate—that is, on purely scientific—principles, we should be among the first to welcome, well knowing that in any issue Science must be the gainer. Although believing in Evolution, we have never given to the hypothesis commonly known as "Darwinism" more than a qualified and provisional adhesion. Whilst admitting that

* Lessons from Nature. By St. George Mivart, F.R.S. London: Murray.

it has thrown a flood of light over some of the most difficult questions in Natural History, and has brought into vital connection a previously incoherent mob of facts, and that it is still a powerful and valuable instrument in the hands of the enquirer, we cannot forget that it has its difficulties. Some of these we have, on former occasions, endeavoured to point out. Hence we should cordially recognise any theory which should either supplement the doctrines of "Natural Selection" and "Sexual Selection," or modify them so as to get rid of their drawbacks and shortcomings. Nay, we should be well pleased to find them superseded altogether by a new hypothesis, adapted at once to the phenomena they have explained and the residues and anomalies which they have hitherto left unsolved. Such a hypothesis we thought Mr. Mivart might have produced, or at least have attempted; and the very attempt could scarcely be made, from a legitimate point of view, without leading to valuable results. Never were we more signally disappointed, although in these days the title of a book is often intended to conceal, rather than to reveal, its nature and object. The strange dedication was, in truth, but too ominous of the contents. The work we found was not constructive, but destructive. It consists of a series of attacks upon a number of men who have done good service in different branches of Science, such as Darwin, Wallace, Huxley, Tyndall, Galton, Lubbock, Helmholtz, Oscar Schmidt,—or who have dealt with methodology, such as Compte, Mill, Spencer, Lewes, &c. The doctrines of Natural Selection and Sexual Selection are indeed discussed, and a desperate effort is made to resuscitate the fast-fading notion of a "great gulf" between man and the lower animals. It is a curious fact that in the old Natural History man is supposed to hold, in relation to other animals, a place very similar to that assigned by the Lavoisierian Chemistry to oxygen in relation to the remaining elements. Unfortunately in biology, passion, prejudice, and sophistry play a more important part than they do in chemistry and physics. The discussion is based upon false principles. We all know the passage in which Mr. Wallace specifies the kind of controversy which alone can be recognised. "As his hypothesis is one which claims acceptance solely as explaining and connecting facts which exist in Nature, he expects facts alone to be brought to disprove it."* This method of discussion finds here comparatively little favour. Theories are tested by their supposed

* Contributions to the Theory of Natural Selection, p. 13.

moral or religious bearings, or by their agreement with the author's *à priori* views. If we bring facts to prove the existence of reason in animals, we are told that we do not know what reason is; if we find in them evidences of moral life, it is said that we have "not even the faintest conception of what a moral nature is." If we show that they possess language, there follows the ready quirk that we confound emotional language with intellectual. That Mr. Mivart's own views of moral nature and of reason must be correct, no one, of course, is supposed to doubt; nor is the spirit of the argument sounder than its method. The author speaks, not as a judge calmly weighing the arguments on either side, and anxious merely that the truth should be ascertained, but as a passionate and eager prosecuting counsel, or rather as a *procureur du roi*, skilfully bringing forward every circumstance, every point—actual or inferred, relevant or irrelevant—which may in any wise damage the defendants, and with equal dexterity concealing whatsoever might tell in their favour. Deep personal hatred towards the "Agnostics" and their doctrines—the *odium theologicum* in its most malignant form—pervades the entire book. Mr. Mivart may doubtless be able to meet Mr. Darwin, Mr. Lewes, Mr. Spencer, or Dr. Huxley, on neutral ground or in private life, on terms of ordinary courtesy; but it is because the man is better and greater than his book. We find here nothing of that fine manly spirit expressed in the old adage—"Plato is my friend, but truth is more my friend." On the contrary, there is one passage in which Mr. Mivart almost seems to apologise for having, on some former occasion, spoken of Mr. Darwin with too much courtesy. For this he has now atoned to an extent almost ludicrous. We should not have felt in the least surprised had we found it proved—of course by strictly metaphysical arguments—that the author of the "Origin of Species" is the veritable transgressor who—

> "Filled the butchers' shops with large blue flies,

or who—

> "With foul earthquakes ravaged the Caraccas,
> And raised the price of sugars and tobaccos."

Suppose, in all sober sadness, an enquirer knowing nothing more of Darwin than what he might learn out of "Lessons from Nature." Would he not go away with the impression that our great English naturalist had done little beyond launching a "puerile hypothesis," and had played a very unimportant—and, if anything, rather injurious—part in the development of biological science? Yet every candid critic

must admit that, were the theory of Natural Selection superseded to-morrow, to Darwin would still belong the merit of effecting in Natural History a transformation as signal as that wrought in astronomy by Galileo, Copernicus, and Kepler, or in chemistry by Lavoisier; of bestowing upon zoology and botany a definite purpose and a direction for research such as before were wanting. His works would still remain a treasury of observations and of suggestions, and the impulse he has given to the Science would never die away. In England, Germany, America, naturalists have sprung up as if by magic in obedience to his spell, and Mr. Mivart himself can hardly be excluded from their number.

We need scarcely add that a critic unjust to persons will not be much more trustworthy as regards their discoveries and their doctrines. The evidence in favour of Natural Selection—and indeed of Evolution altogether—is strictly cumulative, and as such, whatever weight it may carry to the patient and dispassionate enquirer, it is peculiarly open to the attacks of an opponent at once skilful and unscrupulous. We do not, of course, mean to accuse Mr. Mivart of deliberate unscrupulousness. We all know the words —in themselves literally reeking with hypocrisy—in which "the Church" pronounced sentence of death on Giordano Bruno:—*Ut quam clementissime et citra sanguinis effusionem puniretur.*" Yet even on that occasion we should be reluctant to declare that the judges were sinning against better light and knowledge. Just so here: Mr. Mivart doubtless believes and feels what he says, and considers his own line of criticism fair and honourable. We know that man is an adept in self-delusion, and of all men the metaphysician who has cultivated the art *s'égarer avec méthode* is most likely to go unconsciously astray.

We come now to a most painful subject, which, indeed, we would gladly pass over were not its consideration absolutely imperative. Mr. Mivart complains that in one particular instance Mr. Darwin departs from his ordinary courtesy to opponents. We are therefore justified in assuming that he regards courtesy to opponents as a duty—at least in others. Bearing in mind this circumstance we turn to page 144, and read:—"It is in one respect a calamity of our time and country that unbelievers, instead of, as in France, honestly avowing their sentiments, disguise them by studious reticence—as Mr. Darwin at first studiously disguised his views as to the bestiality (!) of man, and as the late Mr. Mill silently allowed himself to be represented to the

public as a thorough believer in God." Along with this passage we take the remarks on "Mr. Winwood Reade, a friend and ardent disciple of Mr. Darwin," and on the teachings of "our English physical expositors" (pp. 393 to 395), and then ask whether the author is not, by implication at least, charging Mr. Darwin with atheism? This is the more probable as we can find no saving clause, or limitation guarding against such a construction being put upon these passages. Still, in a charge so grave the accused is entitled to the benefit of the faintest doubt, and Mr. Mivart may therefore claim a verdict of "Not proven." It is time, however, that we came to a full understanding about the foul practice of introducing charges of atheism in scientific controversy. On this subject we beg to offer the following considerations:—

(1.) Charges of "heresy," "infidelity," or "atheism" are beside the question. If a theory in astronomy, in geology, in physics, chemistry, or biology is in doubt, let it be judged on its own evidence; that is, let it be compared respectively with astronomical, geological, physical, chemical, or biological facts, and, according as it is able or unable to account for and to harmonise such, let it stand or fall. The man who is unable or unwilling to do this convicts himself, from an intellectual point of view, either of impotence or perversity, and should leave controversy to others.

(2.) Such charges, further, are delusive. Not to speak of the thoroughly trained scholar, even many of the "half-educated" know that almost every important discovery in Science has been denounced by the "*parti pretre*" as impious, heretical, and atheistic. A yearly volume of the "Quarterly Journal of Science" would not contain the abuse uttered by ecclesiastics against the Copernican theory of the solar system, against the doctrine of a plurality of worlds, the Newtonian view of the universe, the nebular hypothesis, the chronology of modern geologists, &c. Yet all these views, and many more which might be mentioned, were found—when passion had cooled and sober judgment had time to decide—perfectly compatible, not with theism merely, but with Christian revelation. What "the Church" has cursed in one generation she "assimilates" in the next. What educated man, then, after reviewing the past, can dare to set aside modern theories in such a manner?

(3.) Such charges are, further, distinctly immoral, and even criminal. All civilised countries brand with ignominy the suitor or the advocate who suborns false witnesses, forges or destroys documents, or corrupts judges and juries. But

the controversialist who charges his opponent with atheism stands in a precisely similar position. He well knows that although the public might not admit, *totidem verbis*, that "whatever an atheist advances must be false," or that "every theory once pronounced atheistic must be erroneous," yet it will practically act as if such propositions were established. Hence by making such charges he fraudulently attempts to steal from the public, through an appeal to their passions, a verdict which he has no hope of obtaining from their reason. Knowing and trading on the extreme animosity with which the heretic, the sceptic, and the atheist are —rightly or wrongly—regarded, he seeks to deprive his opponents of a fair hearing by applying to them these dreaded names. A meaner, a more infamous, stratagem can scarcely be conceived. Yet more: it is not the man conscious of the goodness of his cause who fights with such weapons. He who knows that his views are in harmony with facts has nothing to gain by foul play; but if he feels inward misgivings concerning the doctrines which he advocates, or doubts at least the possibility of bringing forward valid arguments in their defence, he may readily, if dishonest enough, seek to blacken the character of an opponent.

We may, therefore, safely and fairly conclude that whosoever in scientific controversy introduces accusations of atheism is, if not knowingly and wilfully, still decidedly in the wrong. We are consequently fully justified in shutting his book, and giving judgment against him.

But there is another consideration which here forces itself upon our attention. All writings calculated to bring a man into general "ridicule, hatred, or contempt," are by the law declared to be libellous. Now it is very questionable if, in England, any accusation is so much calculated to bring a man into "hatred and contempt" as a charge of atheism or "materialism," however ill-founded it may be. Surely therefore such charges, whether brought directly or by implication, are libellous, and as such they are more fitted to be dealt with by a criminal court than by reviewers. We should like to see such a case decided, and we believe that the result would be a great improvement in the tone of scientific and semi-scientific controversy.

But even if such accusations should be pronounced not libellous, and if those who resort to them have no legal penalties to dread, there is another tribunal which might interfere. Why should not scientific men, scientific societies, and scientific journals, agree that whosoever in a scientific controversy attempts to get rid of an opponent by

raising the cry of atheism should be held to be *ipse facto* an outlaw, and to be no longer entitled to the treatment of a gentleman and a scholar? Nay, why should not other charges affecting the personal character of an opponent be dealt with in a similar manner? We do not, of course, seek to screen the man who can be proved to have suppressed documents, cooked results, or claimed as his own discoveries those which he well knew belonged to another. We refer to those random charges of dishonesty and mendacity, and those sweeping ascriptions of motive, which are unfortunately so common. Thus we have often heard and seen it asserted that the authors of some particular theory were actuated by a desire to disprove the existence of a God, to subvert the Christian religion or some particular form of it, or to injure public morals. To such assertors we would reply—"Prove your charge by evidence such as would satisfy an impartial court of justice, or take the consequences, which will not be pleasant!" We are here reminded that in the very passage in Mr. Mivart's book (p. 144) in which he comes unpleasantly near charging Mr. Darwin with atheism, he brings forward against the same gentleman something very like an accusation of dishonesty. It is perfectly true that in the "Origin of Species" Mr. Darwin does not pronounce as to whether mankind had or had not been gradually evolved from some lower form of animal life. But reticence is very different from dishonesty. A thinker is not absolutely bound to bring his speculations to light at all; for keeping them back whilst he is accumulating and weighing the evidence for and against them, he deserves praise rather than censure. Nay, even for introducing doctrines gradually, as the public are able to bear them, there is certainly authority which Mr. Mivart cannot consistently impugn. Nor must we forget that Mr. Darwin has, from the first, nowise courted publicity for his views. But for the fact that Mr. Wallace was known to be preparing a work of a somewhat similar nature, even the "Origin of Species" might never have seen the light.

There may be persons who will be aggrieved at this expression of our views on the subject of scientific controversies; but if they feel themselves guiltless they may cheerfully exclaim—"Let the galled jade wince." As for those who have actually made the kind of charges we protest against, they have no claim to lenity or forbearance.

Controversies on theories in the various inorganic sciences have been carried on with no little acrimony. But charges of atheism are, at least, banished. Why may not this

reform be extended to biology and psychology? Those who cannot treat these subjects from a purely scientific point of view may serve to test the patience of unfortunate reviewers, but they cannot lead us to the truth.

Let us now return to the subject-matter of the controversy before us;—In one passage we find it asserted that the Darwinian theories have met with wide-spread acceptance among the "half-educated." This is quite contrary to our own observation. The most numerous and most virulent opponents of the doctrine of Natural Selection, and indeed of Evolution altogether, are to be found among the following classes:—Retail tradesmen, clerks, shopmen, commercial travellers, "smart" writers in the political press and in purely literary organs, Sunday-school teachers, ministers of the less intellectual dissenting communities, and clergymen who have not had the advantage of a university training. On the other hand, its popularity among working naturalists —"Maenner vom Fach"—is great, and that in proportion as they are working naturalists, men accustomed to deal with things rather than with words or with dreams.

Among the weapons employed against Darwinism a prominent place belongs to the admissions of its author and supporters. But these are almost invariably magnified and distorted, as is often the case, with isolated passages taken out of their connection. If an enquirer avows that his system needs modification, it by no means follows that he abandons it altogether, in any other sense than as we abandon a tentative hypothesis in favour of a closer approximation to the truth. Of the ingenious rather than ingenuous style in which the writings of Evolutionists in general, and of Darwinians in particular, are travestied, we cite the following as a typical instance:—Mr. Darwin having remarked that if man had not been his own classifier the notion of founding a separate order for his own reception would never have arisen, the comment is added;—"That is to say, the irrational classifier would necessarily have excluded the unknown element of reason as a basis of classification!" Mr. Darwin never suggested the possibility of such a contradiction as an "irrational classifier," but assumed animals to be surveyed by a hypothetical being higher than man, or at least totally distinct from him and free from his prepossessions. This "bull"—more absurd, if less humorous, than those of Irish origin—is from "Caliban; the Missing Link," a work written not by Caliban, but by a Mr. D. Watson.

Of course the most satisfactory manner of refuting the

doctrine of Natural Selection must be to supersede it by some better hypothesis, just as the "emission theory" of light was refuted by the production of the "undulatory theory." What, therefore, does Mr. Mivart bring forward to account for the genesis of species? We will take his own words:—"It is quite conceivable that the material organic world may be so constituted that the simultaneous action upon it of all known forces—mechanical, physical, chemical, magnetic, terrestrial, and cosmical,* together with other as yet unknown forces—which probably exist, may result in changes which are harmonious and symmetrical; just as the internal nature of vibrating plates causes particles of sand scattered over them to assume definite and symmetrical figures when made to oscillate in different ways by the bow of a violin being drawn along their edges. The results of these combined internal powers and external influences might be represented under the symbols of complex series of vibrations (analogous to those of sound or light), forming a most complex harmony or a display of most varied colours. In such a way the reparation of local injuries might be symbolised as a filling up and completion of an interrupted rhythm. Thus, also, monstrous aberrations from typical structure might correspond to a discord, and sterility from crossing with the darkness resulting from the interference of waves of light.

"Such symbolism will harmonise with the peculiar reproduction of heads in the body of certain annelids, with the facts of serial homology as well as those of bilateral and vertical symmetry. Also, as the atoms (?) of a resonant body may be made to give out sound by the juxtaposition of a tuning-fork, so it is conceivable that the physiological units of a living organism may be so influenced by surrounding conditions (organic and other) that the accumulation of these conditions may upset the previous rhythm of such units, producing modifications in them,—a fresh chord in the harmony of Nature,—a new species.

Elsewhere he informs us that species arise in virtue of an "internal force or tendency," manifesting themselves "with suddenness, and by modifications appearing at once."

Mr. Mivart therefore does not, with Cuvier and the orthodox naturalists of the old school, maintain that every kind of animal and plant has been separately formed by a distinct

* It is interesting to note the case of "cross division" presented to the reader in this enumeration of forces.

act of Divine intervention, and endowed once for all with its present form, powers, and habits, and has been allotted to some particular district, there to exercise a given function for which it is especially adapted. He is therefore an Evolutionist as decidedly as Lamarck or Darwin, and is necessarily at issue with all who oppose the doctrine of Evolution *in toto*.

Whilst holding that species are mutable, he contends that their changes are not necessarily and invariably gradual, but may have been sudden. Borrowing the terms from geology, he is not a "uniformitarian," but a "catastrophist." The cause of such changes he considers to be not "natural selection," a hypothesis which he dismisses as puerile; not sexual selection; not the influence of changing climate, diet, and other external causes; not to the efforts of animals to adapt themselves to modified circumstances; but to a complex of agencies, internal and external, which might almost be designated "things in general," and of which the author himself, being only able to shadow forth his meaning in metaphorical language, has not, probably, the most distinct conception.

The first objection to Mr. Mivart's views is one which has often been urged against Evolution in general, but which is exceptionally formidable to the theory of sudden modifications. The champions of the Old Zoology are accustomed to say that no change of species has ever yet been actually observed; that animals constantly give birth to young in their own likeness; and that, arguing from the known to the unknown, such must have been the case from the creation of the world, or at least from the dawn of the present order of things, whatever that may mean. To this objection Darwin and Wallace, and all who hold that the difference between species and species has been produced by gradual divergence, have a ready answer. "The variation," they may say, "visible in the life-time of an observer is so trifling as to escape notice." To borrow an illustration from the author of the "Vestiges," as well might an ephemeron deny the development of the frog from the tadpole state, because during his life-time and within the range of the traditions of his ancestors the tadpoles in the pool had remained tailed creatures, breathing through gills. But such a reply is scarcely possible for Mr. Mivart. The appearance of a new mammal, bird, reptile, perhaps we may even add insect, would at once attract attention in any civilised country, often even among barbarians. Can Mr. Mivart adduce an instance in point? We know that species are

continually discovered which are altogether new to Science. But such discoveries are most plentiful—

a. In countries imperfectly explored, becoming rarer and rarer as any region has been more fully explored by naturalists.

b. In the lower forms of animal life, and especially in very minute species.

In England the discovery of a new beetle an inch in length, or of a butterfly the size of *Vanessa Io*, and not obviously imported from some other part of the world, causes no little sensation. On the Continent the occurrence of a nondescript bird or reptile would certainly not be passed over as an every-day affair. Even in India, a buffalo, a deer, or a cat, unknown alike to native and British sportsmen, would excite astonishment. But if such sudden modifications ever have taken place, is it not likely that they would—occasionally at least—still occur, and that they would not be exclusively confined to imperfectly known countries, to microscopic species, and to the lower groups of the animal kingdom? Perhaps Mr. Mivart may say that the "internal forces or tendencies" of species and the external circumstances under which they are placed, have already reacted upon each other, and that no further changes are now possible. We reply that external circumstances continue to alter, and that, consequently, if a perfect equilibrium was at one time attained, the conditions under which it exists being no longer the same, it is liable to be disturbed, thus necessitating on his hypothesis fresh changes. The production of some authenticated case of a new animal or vegetable form evolved out of an old or known one, unessential for Mr. Darwin, is for Mr. Mivart an absolute necessity.

The illustration in which the new hypothesis is conveyed makes, after all, very little room for inward tendencies. The sand, or other powder in which the sound-figures are embodied, lends itself with the same facility to one kind of vibrations as another. The plate, its supports, and the violin-bow are all outward circumstances acting upon the sand. Thus the entire illustration is one which might have been very appropriately used by Lamarck, and in so far forth as it is fully and fairly herein expressed "Mivartism" is merely Lamarckism under a new terminology. Lamarck makes, indeed, no explicit reference to the internal nature of animals; but he must have implicitly assumed it, otherwise there would have been nothing upon which outward circumstances or forces might react. One distinction is, however, that Lamarck, like Darwin, supposed the variation of species

to be gradual, whilst in Mr. Mivart's opinion it may be sudden. But are sudden changes of climate or other outward circumstances sudden? As to the latent internal tendencies they seem to involve greater difficulties and a more frequent recurrence to miracle than the old hypothesis of special creation.

But passing over these minor difficulties we come to the main question—the working of the hypothesis. We have before us certain phenomena, facts, and their relations. A new theory is placed in our hands: how far does it accommodate itself to phenomena? Can we show that it explains what we actually find, whilst if the facts were different they would clash with the theory? Does it give us any hints into what channel we are to direct our observations? Scarcely; it lays before us two unknown powers,—the internal tendencies and the complex of external influences,—and bids us from these deduce the animal kingdom. How are we to discover the magnitude, the direction, the *modus operandi* of either, much less mutual reactions? Surely such a theory is too accommodating, and would lend itself as readily to the monsters of heraldry and the phantoms of mythology as to animals that ever have existed.

Let us once more take Mr. Mivart at his own words, or rather at his own illustration. On the glass disc, then, lie the sound-figures traced in sand, resulting from the last application of the violin-bow. Let it be now applied in a different manner. Instantly, not one, not some, but all of the figures are altered. Translating the symbol into the thing symbolised, this would mean that in a certain organic species—say a butterfly—all the eggs deposited after the new external influences had come into play would yield insects not slightly but abruptly modified, and the old form in a few weeks, or at most months, would entirely disappear. So far this would suit Mr. Mivart perfectly, dissenting as he does from the old maxim that *Natura facit nihil per saltum*, for which he would substitute "*facit multa*," if not "*omnia.*" But how does it agree with facts? On this supposition the rise of a new species would always be attended by the extinction of an old one. Never would a species branch out into two or more, nor would the old form survive the appearance of the new, save in some region to which the modifying influences might not have extended. Thus on Mr. Mivart's principle the multiplication of species, if it took place at all, would be exceedingly slow, and there could be no branching out into a number of closely approximating forms.

On the hypothesis of Natural Selection the process would be very different, and we think more accordant with observation. Suppose a new enemy makes its appearance in the country inhabited by our butterfly before mentioned. One modification of the original stock might escape with relative impunity by reason of superior swiftness; another by being of a shade less easily discerned, or by simulating some more formidable creature; another, perhaps, by being of an evil odour. Thus several species would branch out in different directions, whilst the original type might still exist for some time in gradually decreasing numbers. Thus in one of the very few cases where this Proteus of Mr. Mivart's can be fairly bound, and forced to give a definite reply, the oracle is not in accordance with facts. Whilst, therefore, we confess that Natural Selection has robbed us of no little of the pleasure with which we used to contemplate the animal and the vegetable world, and gladly as we should see it superseded, we cannot pronounce Mr. Mivart's attempt successful, and we doubt whether he is working in the right direction. In any case a vast amount of work requires to be done before his theory can admit even of precise verification. Might we suggest that such work would be infinitely more useful than the metaphysical warfare in which he is now engaged, and would far better merit the title of Lessons from Nature?

The most unsatisfactory, and at the same time the most painfully instructive, portion of the whole work is the attempted demonstration of a "great gulf" between man and the rest of the animal kingdom. The difference he considers as one not of degree, but of kind. Hence he is at issue not merely with Mr. Darwin and the more thorough-going Evolutionist of his immediate school, but with many naturalists who totally reject Evolution. Before Mr. Darwin was known, save as the author of the charming "Voyage of a Naturalist," we had carefully examined the respective position of man and "brutes," and had come to the conclusion that the vulgar doctrine of a great gulf, of a distinction *toto cœlo*, was utterly untenable. We saw that it was one of the lurking remnants of a vicious system of classification which has survived here longer than in other spheres of enquiry, because it panders to man's egotism and vanity. Since then we have met with no facts, no arguments, calculated to subvert our views, but with many, both facts and arguments, by which they are corroborated. It is our full conviction that Mr. Mivart's attempt is a signal failure. His position may be said most nearly to approach that of Swainson; but

the great Quinarian excluded man altogether from the zoological circle proclaiming his structural resemblance to the apes,—relations not of affinity, but merely of analogy, and consequently of no value in determining his rank in the scale of Nature. To him man was not the highest animal, not "an animal and something more," but the lowest, aberrant, member of the spiritual kingdom. Such a doctrine might be hard to substantiate, but it was no less hard to refute, and must at all events be pronounced self-consistent. Mr. Mivart takes up different ground. He admits man to be an animal, but yet proclaims him to be an animal differing more widely from those nearest him in structure, such as the gorilla, than they do from the unorganised lifeless sand beneath their feet. This somewhat sensational deliverance occurs, in substance, more than once, so that it is no mere casual inadvertence. Let us look more clearly into its meaning. Let A denote inorganic matter, B the vegetable world, and C the animal kingdom. In the class C occurs a certain form, *c*, which differs more widely from the other members of the class, *a*, *b*, *d*, &c., than they do from B, or even from A. What kind of classification is this? If *c* differs thus widely from everything else contained in C, we doubt its right to be included in that class at all. Let us take a few instances:—Suppose a curvilinear figure differing more widely from other curvilinear figures than they do in turn from rectilineal figures; suppose a crystal differing more from other crystals than they do from amorphous matter; suppose an acid differing more widely from other acids than they do from bases; suppose a triad differing more widely from other triads than they do from dyads or tetrads; suppose a shade of red differing more widely from other shades of red than they do from yellows or blues; suppose a bird differing more widely from other birds than they do from mammals! Let our readers, if they can, suppose some, any, or all of this, and they will be in a position to understand and appreciate Mr. Mivart's exposition of man's rank in creation. We fear that if any of the "Agnostics" had made a statement half so peculiar it might have received a notice more outspoken than courteous.

Mr. Mivart makes no attempt to base the distinction between man and the lower animals upon points of structure, —in short, upon anything visible. He is far too profoundly versed in animal morphology to make such an attempt. Nay, in a most interesting little work, he has declared that the structure of the frog is by far more isolated and exceptional, with reference to other forms of animal life, than is

that of man. Now, when an army retreats from the open country into quagmires, forests, and deserts, both enemies and neutral on-lookers regard the movement as a confession of weakness. In the very same manner, when a doctrine or a theory changes its ground and recedes, opponents know what this implies. The doctrine of abiogenesis has thus receded. Time was when the world believed that insects of highly complicated organisation could thus be produced. Now it is held, if at all, only with regard to bacteria, invisible to the naked eye. In like manner the doctrine of the "great gulf" was once maintained on points of structure, visible and tangible marks. Now all these supposed characteristics are given up, and the alleged distinction is based on matters invisible—points to be inferred or guessed at. The signification of such a retreat is immense. Mr. Mivart rests his case on a triple assertion :—

Man has language ; brutes have none.
Man has reason ; brutes have merely instinct or *quasi*-intelligence.
Man has an innate perception of right and wrong ; brutes are devoid of moral life.

The three distinctions here brought forward are by no means novel. They have all been previously adopted, and have all in turn been explicitly or tacitly rejected by thinkers who still admit a difference of kind between man and beast. Mivart combines them all, doubtless in the hope that if two wrongs do not make one right, three may possibly be found adequate. We do not find that he is able to bring forward, on any of these points, any argument which may not fairly be considered as already refuted.

The claims of language as a decisive criterion have been urged by Prof. Max Müller—a high authority, doubtless, on human tongues, but, we submit, scarcely so well acquainted with the languages of brutes as to warrant him in pronouncing on the question. Popular opinion, embodied in the phrase "dumb animals," takes a similar view ; but dumb means, after all, little more than speaking a language which we cannot understand. The ancient Greek and the modern Pole both pronounced their neighbours, of different races, "tongueless," or "mute." On the other hand, Quatrefages, a believer in the "great gulf," and a most decided unbeliever in Mr. Darwin, is of opinion that language does not constitute the boundary line. The late Archbishop Whateley was, we believe, of the same opinion.

But turning from authorities, how eminent soever, let us

consider that domestic animals have been found capable of understanding words addressed to them, or merely uttered in their presence, of a more complicated nature than a mere command, and where the tone and gestures of the speaker could supply no clue to the meaning of the speaker.* Now we consider it self-evident that a being absolutely devoid of language, and therefore not fitted for receiving communications from without through any such medium, could at all understand the language of man. Mr. Mivart would probably pronounce all such instances "sensational," and seek to get rid of them by the very compendious process of denial—the way in which inconvenient facts are commonly treated by men of "first principles." We hold, however, that cases of this nature are far too numerous and too well established to be thus summarily dismissed. That the words were actually understood was shown by the events, and the events are generally recorded by observers who had no theory either to defend or to overthrow.

The languages of animals may, doubtless, be poor in abstract terms; but even Prof. Max Müller admits that in human languages abstractions are expressed by words originally concrete in their meaning. Coleridge was of opinion that thought and language were not necessarily connected, and that, had the latter never originated, mankind might have been able to reason without it, and perhaps in a superior manner. The reasoning process in animals may thus be conducted without anything equivalent to words.

It must be further considered that language does establish a break much lower down in the animal kingdom. There are animals which have demonstrable organs of hearing, which possess voices, or instead are endowed with delicate instruments for communicating their meaning by signs. On the other hand, there are other animals which have neither voices nor organs for exchanging signs, nor, as far as we can observe, any auditory apparatus. Surely, then, if we are to take "language" as the test, there is a greater gap, a more complete break, between such absolutely dumb animals and those which can at all events call to each other. Surely there is a wider difference between "nothing" and "something" than between "something" and a greater and more perfect something. The difference of *kind*, according to the language criterion, does not fall between man and apes, but between the higher animals—man included—and certain of the very lowest. We strike out, therefore, at once, the first of Mr. Mivart's three points.

* See Quarterly Journal of Science, v., 70, 71.

What, then, of reason? Is it the exclusive attribute of man? Here, again, we have on our side the suffrages of men perfectly free from the least trace of Darwinian views. Cicero ascribes to the ant—"Mens, ratio et memoria." Milton, a man familiar with metaphysical and scholastic subtleties, makes one of his angels say concerning the lower animals—

"They also reason, not contemptibly."

The orthodox Cuvier, antagonistic as he was to Evolutionism in every guise, speaking of brutes, declares—"Leur intelligence exécute des opérations du même genre." Agassiz holds that we cannot draw any definite boundary between the faculties of a young child and those of a baby-chimpanzee.

We are told, indeed, that were animals rational they would be capable of using language which we could understand. This by no means follows. To us it seems more than merely probable that a great difference in the degree of the mental faculties on either side may be quite sufficient to account for the imperfect understanding that prevails between brutes and ourselves. Perhaps beings as much superior to us as we are to *Acari* may be at this very moment rejecting our claims to reason as flatly as Mr. Mivart rejects those of "our poor relations."

An attempt is also made to show that if working naturalists consider animals to be rational, it is because they do not know what reason is. They ought, forsooth, to study metaphysics, and then might rise to a belief in the great gulf! This suggestion reminds us of the fox, in the fable, who had lost his tail in a trap, and who promised great advantages to his companions if they likewise would submit to amputation. Of course if we allow Mr. Mivart to frame a definition of reason to suit his own objects, the result may be foreseen. We hold that "reason," like "life" or like "poison," may be much more usefully illustrated than defined. We find animals arriving at results similar in nature, though of a lower degree, than what we attain ourselves. We conclude, therefore, that they reach these results not by the aid of a totally different faculty, gratuitously assumed for the occasion, but by a lower grade of the power which we acknowledge in ourselves. Brutes can, as we see, trace effects to their causes; they can devise means to an end under circumstances which forbid recourse to the usual explanation of instinct; they can invent; can be struck with an inward suggestion, can try its feasibility, and put it into

execution. They are even not altogether unable to deal with pure abstractions. Let us take a significant, though very simple, case. Suppose a man required to carry two boxes, not heavy, but too bulky to be conveniently grasped at once. A "happy thought" strikes him; he examines them, compares their sizes, finds that one can be conveniently nested within the other, and completes his task with ease. This case, simple as it is, manifestly involves reason. Nay, most of our readers will have met with men who, when they encounter some such difficulty, seem incapable of devising any expedient to escape it. What, then, must we say of the dog referred to in the following case?—"One of the most unmistakable examples of dog-reason I can call to mind is that of a Newfoundland dog sent across a stream to fetch a couple of hats, whilst his master and a friend had gone on some distance. The dog went after them, and the gentlemen saw him attempt to carry both hats, and fail, for the two were too much for him. Presently he paused in his endeavour, took a careful survey of the hats, discovered that one was larger than the other, put the small one in the larger, and took the latter in his teeth by the brim." * Or, again, suppose that a savage observes game frequenting a certain track in a forest. A suitable locality suggests to him the possibility of catching them by a stratagem. He makes an experiment to test the practicability of the scheme, and feeling satisfied on this score, puts it in successful operation. Were such a case narrated it would be at once accepted as a proof of reason in the savage, and might be made the subject of much sensational comment. Yet here is the very action performed not by "a man and a brother," but by a fox:—"On coming home from shooting I observed, at some distance, a fox jumping continually up to a trunk of a tree of a middling height, holding something in his mouth. On examination I saw it to be a branch of a considerable size. Anxious to learn the reason I laid myself quietly down. In a very short time the fox laid down the branch and sat down on the trunk, prepared for a jump. Soon after I heard the approach of a family of wild pigs, which after some time were quite near to the stump. At the moment when they passed the fox, he jumped down on one of the young pigs, and returned with it to his elevated perch, preparing himself to begin a fat breakfast, quite careless of the impotent anger of the wild sow."† With the man who can venture to refer

* Shirley Hibberd, Clever Dogs, &c.
† *Zoologist*, p. 1365.

this case to instinct it is a mere waste of time to argue. Those who dispute the fact are reminded that lions have been seen in a very similar manner practising a manœuvre, and even conferring together on the subject.*

The following fact, which has never been questioned, is a clear case of a discovery made by insects, and forthwith turned to practical account:—Ants have been observed, both by Réaumur and Bonnet, to place their eggs between the outer wooden casing and the inner panes of a glass bee-hive, a situation where, without any trouble on their part, a regular and sufficient temperature exists. By so doing they are enabled to dispense with a great amount of labour in removing the eggs from one part of the nest to another, according to the weather. On this subject Messrs. Kirby and Spence remark†—"It is impossible to account for this without supposing some stray ant that had insinuated herself into this tropical crevice first to have been struck with the *thought* of what a prodigious saving of labour and anxiety would accrue to her compatriots by establishing their society here; that she had communicated her views to them, and that they had resolved upon an emigration to this newly-discovered country, whose genial climate presented advantages which no other situation could offer. Neither instinct nor any conceivable modification of instinct could have taught the ants to avail themselves of a good fortune which, but for the invention of glass hives, would never have offered itself to these insects. The conclusion seems irresistible that reason must have been their guide, inducing a departure from their ordinary habits." We may here ask—If observation and subsequent reflection can induce an animal to depart from its ordinary habits, are not those habits themselves under the direction of reason?

One case more, typical of a very important class, must be brought forward. Number, it will be conceded, is an abstract idea. A work was written but a little while ago‡ to prove the inability of animals to comprehend even the simplest numerical relations. There is, however, an instance on record of a Scotch collie, who, when assisting at the operation of sheep-washing, showed himself equal to count quite as well as many savages. There was close to the stream a small pen, capable of holding, if we remember rightly, eleven sheep at a time. The dog, without any assistance, always started off to the flock and drove up the

* R. Moffat, Missionary Labours and Scenes in Southern Africa.
† Kirby and Spence, Entomology, ii., p. 416.
‡ See Quarterly Journal of Science, v., 361.

sheep in successive lots of eleven, without ever committing an error. We are unable to see how even the most adroit sophist can explain away this case. Man, however, is very loth to yield his fancied superiority. If the actions of animals can no longer be all explained by "instinct," surely some new name can be invented! Words are very cheap, and if they signify nothing where is the harm? Accordingly we have a new set of faculties, to which the actions of brutes may be ascribed. We hear of "*quasi*-intelligence," "*quasi*-mind," and even of "*quasi*-memory." Perhaps we shall in due time be informed that when an animal is in need of food it feels "*quasi*-hunger," and that when overdriven it suffers from "*quasi*-fatigue." Is it not gratuitous and unphilosophical in the extreme thus to multiply imaginary faculties? If it can be positively proved, from facts, that a dog remembers persons, places, or events by a totally different process and on totally different principles from what we ourselves do, then it will be time to talk of "*quasi*-memory." Until such proof is furnished it is a mere insult to our common sense. More than that, it is the *reductio ad absurdum* of all systems and first principles from which such a conclusion can be drawn. We trust that our physicists and chemists may not catch this infection, and treat us to *quasi*-magnetism, *quasi*-light, and *quasi*-gravitation.

It is suggested that a book should be written on the stupidity of animals. Such a work might then be very appropriately followed up by a companion volume on the stupidity of mankind. We fear that the latter, if fairly compiled, would prove the bulkier of the two. We are told that an elephant at the Zoological Gardens, finding the end of its trunk entangled in a ring, pulled till it tore off the extremity of its own member; but we know of a man who, in pruning his orchard, deliberately and neatly sawed away the branch against which his ladder was leaning, and fell to the ground with great violence. His name was Ferdinand Hilthel, and he lived not fifteen miles from Goerlitz. Yet on the strength of such negative cases we should not be justified in pronouncing man irrational. Why then should such an inference be drawn from the occasional, or even frequent, stupidity of the lower animals?

As a proof of animal irrationality it is said that a dog has been known, in a sudden broil, to fly *at* his master. We do not in the least dispute it, for we have seen very similar blunders committed by man! We witnessed an instance where a gentleman, in the confusion consequent upon a railway-train arriving much behind its time at a crowded

station, actually sought to prevent his wife from getting into the same compartment as himself, whilst all the time he was most anxious to secure a seat for her. Are we to pronounce him endowed merely with "*quasi*-intelligence"?

We may surely decide that the attempt to erect "reason" into an absolute criterion for distinguishing between man and beast is an utter failure, and that its hopelessness will be more and more recognised the more profound and the more accurate our knowledge of the animal world becomes. Vast as is the superiority of our own species over even the highest brute, the difference is not of kind, but of degree.

We cannot help pointing out as significant the assertion that if the ants could be proved to be rational they would be insects merely in form!

We pass on to the last remaining point, the moral life, and ask if here can be found the absolute distinction which, mirage-like, has fled as we have followed? One of the charges brought against the so-called "Agnostics" is that they confound virtue with pleasure. This accusation is urged in a chapter in which he seeks to show that "perceptions of right and wrong, and of our power of choice and consequent responsibility, are universally diffused amongst mankind, and constitute an absolute character separating man from all other animals." Here, as usual, dissidents are criticised sometimes singly and sometimes collectively, all being, by implication at least, held answerable for any error or oversight, real or imaginary, detected in the writings of any one, and for its assumed consequences. Now, that some modern writers may have forgotten that an action highly pleasurable to the doer is not necessarily virtuous, we shall not seek to deny. In so doing they have been probably influenced by a more or less conscious reaction against the opposite error so dominant in the Dark Ages, and at all other times of rampant ecclesiasticism—that an action was to be regarded as vicious in the exact proportion of its pleasurableness, even though no person were injured, whilst sufferings and privations by which no one was benefitted were deemed virtuous and meritorious. Of these two opposite errors the modern one is assuredly the less dangerous. But it is very curious that Mr. Mivart, well acquainted as he must be with the writings of Mr. Herbert Spencer, has not thought proper to allude to his criterion for distinguishing evil from good. To this we are therefore obliged to call attention:—"From whatever assumptions they start, all theories of morality agree that conduct whose total results, immediate and remote, are beneficial, is good

conduct; whilst conduct whose total results, immediate and remote, are injurious. is bad conduct." * This passage, we submit, completely refutes the charge brought against modern philosophy—that it teaches man to indulge any desire, no matter at what cost to others, or with what future consequences to himself, Ultimate as well as immediate results are considered, and the effects—not upon the actor alone, but upon all persons whom the action can possibly influence—are fully weighed.

To Mr. Spencer's criterion it has been objected that it leaves motives out of the question. But we are not sure that the consideration of motives can be, generally speaking, anything but delusive. What may be the motives which have led to any particular line of conduct we may guess with more or less of correctness, but we can never know with certainty. Nor in many cases can motives, however accurately known, be allowed to weigh in the same scale as results, or in any manner to affect our judgment. We should surely not show any more mercy to a Thug, or to a judge of the "Holy Office," who murders men "for their soul's health" or for the pretended honour of his God, than we would to a Greek brigand or a Chinese pirate. If anything, the latter are the less dangerous criminals. Mr. Mivart dispenses with every criterion, holding man's notions of right and wrong to be intuitive: his manner of dealing with the innumerable facts that prove, on the contrary, that man has no such innate standard, is saddening. The strangest scepticism, on the one hand, is blended with a credulity no less strange on the other. The fact that no authenticated instance of remorse on the part of a savage can be adduced is left in the background. The outrages of wild men are sought to be explained away with a wonderful amount of misplaced ingenuity. We quote the following passage:—"Thus the most revolting act that can well be cited, that of the deliberate murder of aged parents, monstrous as the act in itself is, may really be one of filial piety, if, as is asserted, the savage perpetrators do it at the wish of such parents themselves, and from a conviction that thereby they not only save them from suffering in this world, but also confer upon them prolonged happiness in the next." It is a known fact that the murder of aged relatives is often effected in such a way that the victims would never solicit it as a favour. Sometimes they are disposed of not by being promptly slain, but by being simply abandoned to die of

* Essay on Education, p. 114.

hunger and thirst, or to be devoured by wild beasts, as the chance may be. Can we believe that any person would wish to be thus "saved from suffering in this world"? But we need not confine our attention to the lowest savages. The ancient Danes, as is very well known, had a custom of tossing young children upon the points of their spears. Was this act, also, a species of disguised kindness, intended merely for the good of the victims? Surely an innate moral sense which can allow such actions as we have here mentioned, not to speak of what might further be brought forward, must be of no practical value. It is idle to say that savages do not approve of murder, robbery, and outrage, because they become angry if themselves or their tribe are the sufferers. The lower animals do just the same: rob a wild beast of his prey, or of his young, and your life is in peril. Shoot a peccary, and the whole tribe rushes upon you.

Mr. Mivart quotes as an example of "Savage refinement" the following passage from Sir John Lubbock:—

"Among the Greenlanders, should a seal escape with a hunter's javelin in it, and be killed by another man afterwards, it belongs to the former. But if the seal is struck with the harpoon and bladder and the string breaks, the hunter loses his right. If a man finds a seal dead with a harpoon in it, he keeps the seal but returns the harpoon. Any man who finds a piece of drift-wood can appropriate it by placing a stone on it as a sign that some one has taken possession of it. No other Greenlander will then touch it."*

This is very interesting; but we can give from our own observation a case somewhat similar amongst animals certainly not ranking high in the scale of intelligence. We kept formerly a number of vipers and other snakes in a pit something like a melon frame. If a live mouse was dropped into the pit there was a general scramble, and all the venomous inmates were seen snapping at the warm-blooded intruder. But as soon as one had planted a fatal bite all the others withdrew into their crevices, or coiled themselves up to sleep, leaving the conqueror to the quiet enjoyment of his meal. This we witnessed repeatedly, and can bear witness that it was not the largest or strongest snakes alone whose rights to their prey were thus left undisputed.

But even if it were shown that all existing tribes on the earth had some notions of morality the question is still open. Mr. Mivart assumes that no tribes ruder and lower than any now dwelling upon our globe have flourished in primeval

* Origin of Civilisation, p. 305.

days, and have been swept away. Yet that such must have once existed will appear highly probable if we reflect on the process of extermination still going on. Is it not likely that in more barbarous days before modern philanthropy had arisen, and before the Aborigines Protection Society had been organised, wars of extirpation, always directed against the races least raised above the brute level, would be more frequent and more destructive? The legends of all ancient nations point in the same direction, telling us of half-human monsters whom their forefathers extirpated or drove out.

But this is not all; before the presence of a moral sense, of a feeling of right and wrong, whether innate or acquired, can be brought forward as "an absolute characteristic separating man from all other animals," it must be shown that no other animal possesses such moral sense. And here Mr. Mivart has nothing save baseless, wanton assumption to array against solid facts. He may, if it so please him, *assert* that brutes are void of all traces of conscience, but unless he could enter into their minds and be aware of their feelings, such assertion is unwarrantable. Suppose a mineralogist were to hold up before us two minerals, A and B, very similar in all their outward properties, and should inform us that the distinction between them consisted in the fact that cobalt was always present in A, and was as uniformly absent in B. We should naturally ask if he had analysed both and could give a full account of all their constituents? What should we think were he to reply:—"I certainly have analysed A, and have found the presence of cobalt. B I have not been able to analyse, but on *a priori* grounds I am satisfied that no cobalt can there be present." Should we not feel inclined to send him back to take some quite other "lessons from nature?"

As an instance of the very different conclusions which other minds have drawn from a close and prolonged observation of animal life, we may take the following passage from the writings of the late Agassiz. Pronouncing the range of passion in animals as extensive as in man, he continues:— "I am at a loss to trace a difference of kind between them. The gradation of moral faculties between the higher animals and man is so imperceptible that to deny to the first a certain sense of consciousness and responsibility would be an exaggeration." These are the words, it must be remembered, of an original observer of unquestioned ability and untiring industry, who, moreover, devoted his attention far more closely and exclusively to biology than Mr. Mivart has

apparently done. Agassiz, further, was no "Agnostic," no Darwinist, no believer in Evolution, but a champion of the doctrine of individual creation. No one can accuse him of seeking to under-estimate the difference between man and other animals from any sinister motive.

The Rev. J. G. Wood, in his recent interesting work "Man and Beast,"—without, as far as we can perceive, accepting Darwinism, or even Evolution, and certainly without seeking to demonstrate our kinship with apes,—arrives at conclusions closely resembling those of Professor Agassiz, and even produces no contemptible evidence in favour of animal immortality. The like has been done by Bishop Butler. Nor can it be denied that some at least of the strongest arguments advanced in favour of man's immortality tell in favour of a hereafter in store for lower animals. If the life of man is a drama, of which the fifth act, with its compensations and retributions, is reserved for another stage, surely the same should hold good with brutes, among whom also there prevail those differences of destiny which have perplexed man.

There are on record fully authenticated instances of animals feeling ashamed of actions they have committed. We may refer to the case observed and described by Mr. G. J. Romanes.* This case, which we think no one will attempt to ignore as the exaggeration or the mistake of an incompetent observer, is very significant. To escape ridicule X is tempted to tell (or not) a falsehood. Detected in this the said X feels much more distressed and ashamed than when merely ridiculed for his blundering. Now if for X we read John Nupkins, all will admit that John Nupkins knew that falsehood was wrong, and will call his subsequent distress the action of a guilty conscience. But if, instead of John Nupkins, X happens—as in this case—to stand for a terrier, where is our right to put any different interpretation on the same set of facts?

Among certain birds—*e.g.*, rooks—careful observers have detected distinct traces of criminal law. Thievish birds, who persevere in stealing sticks from the nests of their fellow-citizens, have been seen banished from the community, severely chastised, and even killed, by a general assemblage. "But law necessarily pre-supposes the notions of right and wrong, and could never, therefore, have arisen among beings incapable of drawing this distinction."

We shall add one more case to prove in the lower animals

* See Quarterly Journal of Science, v., 425., and p. 153 of present number.

the existence of free will, the power of overcoming natural instincts and temptations, in order to secure a supposed benefit in the sequel ;—" A fine terrier, in the possession of a surgeon at Whitehaven, about three weeks ago exhibited its sagacity in a rather amusing manner, It came into the kitchen and began plucking the servant by the gown, and, in spite of repeated rebuffs, it perseveringly continued in its purpose. The mistress of the house, hearing the noise, came down to enquire the cause, when the animal treated her in a similar manner. Being struck with the concern evinced by the creature, she quietly followed it upstairs into a bed-room, whither it led her ; there it commenced barking, looking under the bed, and then up in her face. Upon examination, a cat was discovered there quietly demolishing a beef-steak, which it had feloniously obtained. The most singular feature in the whole case is that the cat had been introduced into the house only a short time before, and that bitter enmity prevailed between her and her canine companion."* This is a capital case. " Instinct " would undeniably have led the terrier to attack the cat and attempt to deprive her of her booty, the rather as the two animals were on unfriendly terms. But we find this natural impulse here completely restrained for the attainment of a certain definite end. The terrier lays an information against his enemy. Why should he, unless he entertained the notion that theft was wrong ? He evidently concluded that his enemy, if detected in such an act, would probably suffer severe punishment. The incident is of the greater value as it prove that brutes are capable not merely of planning means to effect an object quite unconnected with the preservation of the individual or of the species, but of exercising self-control ; that, in short, they do not always blindly and necessarily follow their physical appetites, but can, like man, forego present indulgence for what appears to them a greater good hereafter.

A strange attempt is made to show that animals are altogether unconscious ; that though they feel pain, they are not aware of so doing. They are represented as being, therefore, naturally and permanently in the state into which man may be artificially and temporarily thrown by means of anæsthetics. If this be, then cruelty to animals is an impossibility, and the stipulation that in vivisection anæsthetics are to be employed is farcical in the extreme. But what of the evidence for this assumption ? We know that man can

* Zoologist," p. 2131.

be conscious of suffering, and also, under certain circumstances, unconscious. We are not without grounds for supposing that the lower animals feel less acutely than he does.* But how are we to be certain that as a class they are always unconscious of pain? Who has looked into the mind of a tortured horse or dog, and satisfied himself that it was unconscious of its misery, and therefore not miserable? And how is the favoured sage who has done this wondrous thing to satisfy us that he has observed and interpreted aright? Mr. Mivart tells us that a wasp deftly snipped in two with a pair of scissors, whilst sipping honey or syrup, will continue its banquet. We have seen an impaled dragon-fly greedily devour a blue-bottle presented to his jaws. The absence of struggle and disturbance is here taken as a proof that the animal does not feel. But if so, when we find a wounded animal writhing and screaming, may we not infer that it both feels and knows that it feels?

What of mental distress, sorrow, as distinct from bodily pain? We know that this condition produces in brutes the very same results as in man. Dogs have been known to pine away and die for the loss of their masters. Female apes sometimes, "sensational" as it may seem, do not always recover from the effects of losing their infants. Birds have often drooped and died on losing their mates. A horse sometimes falls out of condition on parting with its yoke-fellow. Yet we are to believe that they are all the while unconscious of the distress they suffer! In short, for the notion of the total unconsciousness of animals, we can find no valid evidence, but much against it, and must therefore dismiss it to limbo as one of the many far-fetched and hopeless attempts to defend the "great gulf."

We do not consider it legitimate to denounce any scientific hypothesis because some persons may find in it countenance for moral or social errors. But we cannot help pointing out that, had this unconsciousness of animals been advanced as a cardinal point of Darwinism, great would have been the outcry raised against the demoralising tendencies of modern science. Here, however, science pleads on the side of mercy, whilst Mediævalism replies to every attempt to lighten the sufferings of domestic animals: "No es Cristiano!"

The whole work, despite the unquestionable ability which it evinces, must be pronounced disappointing—worthy, perhaps, of a Joseph de Maistre, but utterly unworthy of a

* It is probable also that the lower races, or species—if we may venture to use the word—of mankind have less feeling than the higher.

Mivart. If such are genuine "Lessons from Nature" the venerable dame keeps a most inefficient school, and the sooner it is closed the better. But in this case the pure white light has passed through such a powerfully distorting medium that its true nature can scarcely be recognised. Metaphysics may be regarded as a disease which thinkers are liable to contract at some part of their career, just as children take the measles, or rather the scarlet fever. Mr. Mivart's is a very bad case; but for his own sake, and still more for that of Science, we wish him a full and a speedy recovery. An intellect like his is too valuable to be lost.

V. THE MECHANICAL ACTION OF LIGHT.*

By W. Crookes, F.R.S.

To generate motion has been found a characteristic common, with one exception, to all the phases of Physical Force. We hold the bulb of a thermometer in our hands, and the mercury expands in bulk, and, rising along the scale, indicates the increase of heat it has received. We heat water, and it is converted into steam, and moves our machinery, our carriages, and our ironclads. We bring a loadstone near a number of iron filings, and they move towards it, arranging themselves in peculiar and intricate lines; or we bring a piece of iron near a magnetic needle, and we find it turned away from its ordinary position. We rub a piece of glass with silk, thus throwing it into a state of electrical excitement, and we find that bits of paper or thread fly towards it, and are, in a few moments, repelled again. If we remove the supports from a mass of matter it falls, the influence of gravitation being here most plainly expressed in motion, as shown in clocks and water-mills. If we fix pieces of paper upon a stretched string, and then sound a musical note near it, we find certain of the papers projected from their places. Latterly the so-called "sensitive flames," which are violently agitated by certain musical notes, have become well known as instances of the conversion of sound into motion. How readily chemical force undergoes the same transformation is manifested in such catastrophes as those of Bremerhaven, in the

* A Lecture delivered at the Royal Institution, on Friday evening, February 11th, 1876.

recent deplorable coal-mine explosions, and indeed in every discharge of a gun.

But light, in some respects the highest of the powers of Nature, has not been hitherto found capable of direct conversion into motion, and such an exception cannot but be regarded as a singular anomaly.

This anomaly the researches which I am about to bring before you have now removed; and, like the other forms of force, Light is found to be capable of direct conversion into motion, and of being—like heat, electricity, magnetism, sound, gravitation, and chemical action—most delicately and accurately measured by the amount of motion thus produced.

My research arose from the study of an anomaly.

It is well known to scientific men that bodies appear to weigh less when they are hot than when they are cold; the explanation given being that the ascending currents of hot air buoy up the body, so to speak. Wishing to get rid of this and other interfering actions of the air during a research on the atomic weight of thallium, I had a balance constructed in which I could weigh in a vacuum. I still, indeed, found my apparatus less heavy when hot than when cold. The obvious explanations were evidently not the true ones: *obvious* explanations seldom are true ones, for simplicity is not a characteristic of Nature.

An unknown disturbing cause was interfering, and the endeavour to find the clue to the apparent anomaly has led to the discovery of the mechanical action of light.

I was long troubled by the apparent lawlessness of the actions I obtained. By gradually increasing the delicacy of my apparatus I could easily get certain results of motion when hot bodies were brought near them, but sometimes it was one of attraction, at others of repulsion, whilst occasionally no movement whatever was produced.

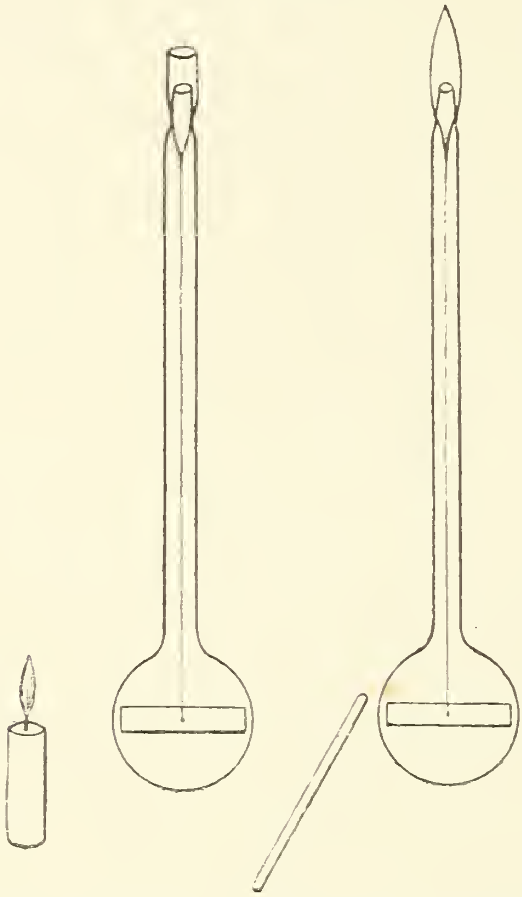
I will try to reproduce these phenomena in this apparatus (Fig. 1). Here are two glass bulbs, each containing a bar of pith about 3 inches long and ½ an inch thick, suspended horizontally by a long fibre of cocoon silk. I bring a hot glass rod, or a candle, towards one of them, and you see that the pith is gradually attracted, following the candle as I move it round the bulb. That seems a very definite fact; but look at the action in the other bulb. I bring the candle, or a hot glass rod, near the other bar of pith, and it is strongly *repelled* by it—much more strongly than it was attracted in the first instance.

Here, again, is a third fact. I bring a piece of ice near the pith bar which has just been repelled by the hot rod,

and it is attracted, and follows the rod round as a magnetic needle follows a piece of iron.

The repulsion by radiation is the key-note of these researches. The movement of a small bar of pith is not very distinct, except to those near, and I wish to make this repulsion evident to all. I have therefore arranged a piece of apparatus by which it can be seen by all present. I will, by means of the electric light, project an image of a pendulum suspended *in vacuo* on the screen. You see that the approach of a candle gives the bob a veritable push, and, by alternately obscuring and uncovering the light, I can make the pendulum beat time to my movements.

FIG. I.

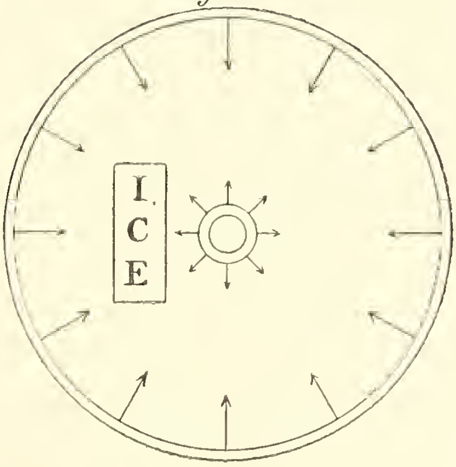


What then is the cause of the contradictory action in these two bulbs—attraction in one, and repulsion in the other? It can be explained in a few words. Attraction takes place when air is present, and repulsion when air is absent.

Neutrality, or no movement, is produced when the vacuum is insufficient. A minute trace of air in the apparatus interferes most materially with the repulsion, and for a long time I was unaware of the powerful action produced by radiation in a "perfect" vacuum.

It is not at first sight obvious how ice or a cold body can produce the opposite effect to heat. The law of exchanges, however, explains this perfectly. The pith bar and the whole of the surrounding bodies are incessantly exchanging heat-rays; and under ordinary circumstances the income and expenditure of heat are in equilibrium. Let me draw your attention to the diagram (Fig. 2) illustrating what takes place when I bring a piece of ice near the apparatus. The centre circle represents my piece of pith; the arrows show the influx and efflux of heat. A piece of ice brought near cuts off the influx of heat from one side, and therefore allows an excess of heat to fall on the pith from the opposite side. Attraction by a cold body is therefore seen to be only repulsion by the radiation from the opposite side of the room.

Fig 2.



The later developments of this research have demanded the utmost refinement of apparatus. Everything has to be conducted in glass vessels, and these must be blown together till they make one piece, for none but fused joints are admissible. In an investigation depending for its successful prosecution on manipulative dexterity, I have been fortunate in having the assistance of my friend Mr. Charles Gimingham. All the apparatus you see before you are the fruits of his skilful manipulation, and I now want to draw your attention to what I think is a masterpiece of glass-working —the pump which enables me so readily to produce a vacuum unattainable by ordinary means.

The pump here at work is a modification of the Sprengel pump, but it contains two or three valuable improvements. I cannot attempt to describe the whole of the arrangements, but I will rapidly run over them as illuminated by the electric light. It has a triple fall tube in which the

mercury is carried down, thus exhausting with threefold rapidity; it has Dr. McLeod's beautiful arrangement for measuring the residual gas; it has guages in all directions, and a small radiometer attached to it to tell the amount of exhaustion that I get in any experiments; it has a contrivance for admitting oil of vitriol into the tubes without interfering with the progress of the exhaustion, and it is provided with a whole series of most ingenious vacuum-taps devised by Mr. Gimingham. The exhaustion produced in this pump is such that a current of electricity from an induction-coil will not pass across the vacuum. This pump is now exhausting a torsion-balance, which will be described presently. Another pump, of a similar kind but less complicated, is exhausting an apparatus which has enabled me to pass from the mere exhibition of the phenomena to the obtaining of quantitative measurements.

A certain amount of force is exerted when a ray of light or heat falls on the suspended pith, and I wished to ascertain—

First. What were the actual rays—invisible heat, luminous, or ultra violet—which caused this action?

Secondly. What influence had the colour of the surface on the action?

Thirdly. Was the amount of action in direct proportion to the amount of radiation?

Fourthly. What was the amount of force exerted by radiation?

I required an apparatus which would be easily moved by the impact of light on it, but which would readily return to zero, so that measurements might be obtained of the force exerted when different amounts of light acted on it. At first I made an apparatus on the principle of Zöllner's horizontal pendulum. For a reason that will be explained presently I am unable to show you the apparatus at work, but the principle of it is shown in the diagram (Fig. 3). The pendulum represented by this horizontal line has a weight at the end. It is supported on two fibres of glass, one stretched upwards and the other stretched downward, both firmly fastened at the ends, and also attached to the horizontal rod (as shown in the figure) at points near together, but not quite opposite to one another.

It is evident that if there is a certain amount of pull upon each of these fibres, and that the pull can be so adjusted as to counteract the weight at the end and keep it horizontal, the nearer the beam approaches the horizontal line the slower

its rate of oscillation. If I relax the tension, by throwing the horizontal beam downwards, I get a more rapid oscillation sideways. If I turn the levelling screw so as to raise the beam and weight, the nearer it approaches the horizontal position the slower the oscillation becomes, and the more delicate is the instrument. Here is the actual apparatus that I tried to work with. The weight at the end is a piece of pith; in the centre is a glass mirror, on which to throw a ray of light, so as to enable me to see the movements by a luminous index. The instrument, enclosed in glass and exhausted of air, was mounted on a stand with levelling screws, and with it I tried the action of a ray of light falling on the pith. I found that I could get any amount of sensitiveness that I liked; but it was not only sensitive to the impact of a ray of light, it was immeasurably more so to a change of horizontality. It was in fact too delicate for me to work with. The slightest elevation of one end of the instrument altered the sensitiveness, or the position of the zero point, to such a degree that it was impossible to try any experiments with it in such a place as London. A person stepping

Fig. 3.

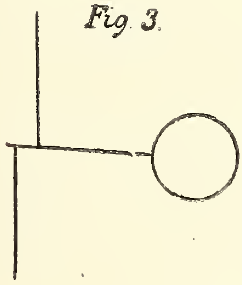
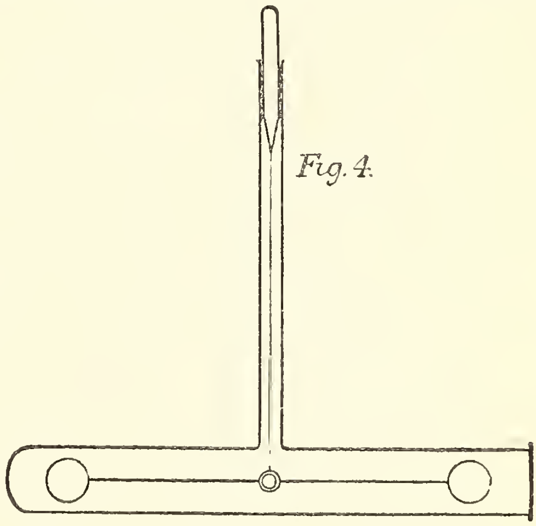


Fig. 4.



from one room to another altered the position of the centre of gravity of the house. If I walked from one side of my own laboratory to the other, I tilted the house over sufficiently to upset the equilibrium of the apparatus. Children playing in the street disturbed it. Prof. Rood, who has worked with an apparatus of this kind in America, finds that an elevation of its side equal to 1-36,000,000th part of an inch is sufficient to be shown on the instrument. It was therefore out of the question to use an instrument of this construction, so I tried another form (shown in Fig. 4), in which a fine glass beam, having discs of pith at each end, is suspended horizontally by a fine glass fibre, the whole

being sealed up in glass and perfectly exhausted. To the centre of oscillation a glass mirror is attached.

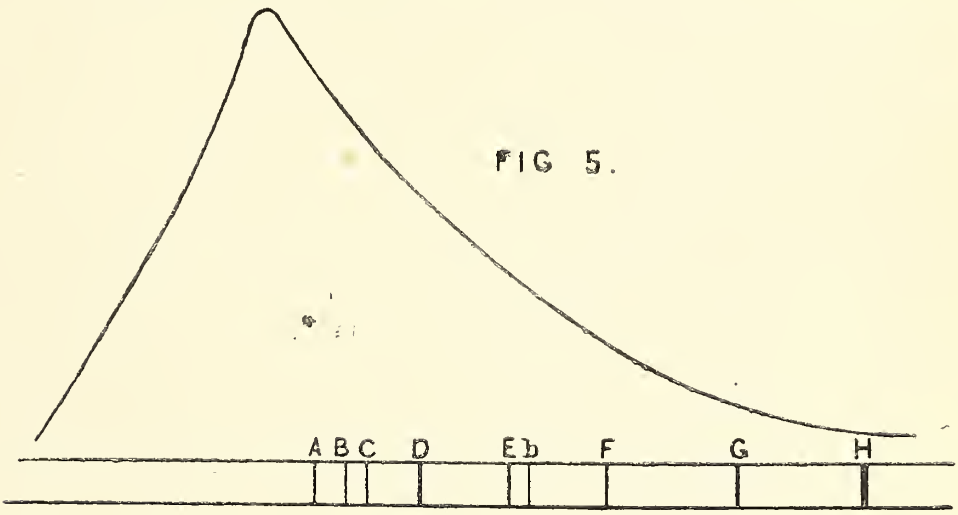
Now a glass fibre has the property of always coming back to zero when it is twisted out of its position. It is almost, if not quite, a perfectly elastic body. I will show this by a simple experiment. This is a long glass fibre hanging vertically, and having a horizontal bar suspended on it. I hold the bar, and turn it half round; it swings backwards and forwards for a few times, but it quickly comes back to its original position. However much twist, however much torsion, may be put on this, it always returns ultimately to the same position. I have twisted glass fibres round and kept them in a permanent state of twist more than a hundred complete revolutions, and they always came back accurately to zero. The principle of an instrument that I shall describe farther on depends entirely on this property of glass.

Instead of using silk to suspend the torsion beam with, I employ a fibre of glass, drawn out very fine before the blowpipe. A thread of glass of less than the thousandth of an inch in thickness is wonderfully strong, of great stiffness, and of perfect elasticity, so that however much it is twisted round short of the breaking point, it untwists itself perfectly when liberated. The advantage of using glass fibres for suspending my beam is, therefore, that it always returns accurately to zero after having tried an experiment, whilst I can get any desired amount of sensitiveness by drawing out the glass fibre sufficiently fine.

Here, then, is the torsion apparatus sealed on to a Sprengel pump. You will easily understand the construction by reference to the diagram (Fig. 4). It consists of a horizontal beam suspended by a glass fibre, and having discs of pith at each end coated with lamp-black. The whole is enclosed in a glass case, made of tubes blown together, and by means of the pump the air is entirely removed. In the centre of the horizontal beam is a silvered mirror, and a ray from the electric light is reflected from it on to a scale in front, where it is visible as a small circular spot of light. It is evident that an angular movement of the torsion beam will cause the spot of light to move to the right or to the left along the scale. I will first show you the wonderful sensitiveness of the apparatus. I simply place my finger near the pith disc at one end, and the warmth is quite sufficient to drive the spot of light several inches along the scale. It has now returned to zero, and I place a candle near it. The spot of light flies off the scale. I now bring the candle near it alternately from one side to the

other, and you see how perfectly it obeys the force of the candle. I think the movement is almost better seen without the screen than with it. The fog, which has been so great a detriment to every one else, is rather in my favour, for it shows the luminous index like a solid bar of light swaying to and fro across the room. The warmth of my finger, or the radiation from a candle, is therefore seen to drive the pith disc away. Here is a lump of ice, and on bringing it near one of the discs the luminous index promptly shows a movement of apparent attraction.

With this apparatus I have tried many experiments, and amongst others I endeavoured to answer the question "Is it light, or is it heat, that produces the movement?"—for that is a question that is asked me by almost everyone;



and a good many appear to think that if the motion can be explained by an action of heat, all the novelty and the importance of the discovery vanish. Now this question of light or heat is one I cannot answer, and I think that when I have explained the reason you will agree with me that it is unanswerable. There is no physical difference between light and heat. Here is a diagram of the visible spectrum (Fig. 5). The spectrum, as scientific men understand it, extends from an indefinite distance beyond the red to an indefinite distance beyond the violet. We do not know how far it would extend one way or the other if no absorbing media were present; but, by what we may call a physiological accident, the human eye is sensitive to a portion of the spectrum situated between the line A in the red to about the line H in the violet. But this is not a physical difference between the luminous and non-luminous parts of the spectrum; it is only a physiological difference. Now, the part at the red end of the spectrum possesses, in the greatest degree, the property of causing

the sensation of warmth, and of dilating the mercury in a thermometer, and of doing other things which are conveniently classed among the effects of *heat;* the centre part affects the eye, and is therefore called *light;* whilst the part at the other end of the spectrum has the greatest energy in producing *chemical action.* But it must not be forgotten that any ray of the spectrum, from whatever part it is selected, will produce all these physical actions in more or less degree. A ray here, at the letter C for instance in the orange, if concentrated on the bulb of a thermometer, will cause the mercury to dilate, and thus show the presence of *heat;* if concentrated on my hand I feel *warmth;* if I throw it on the face of a thermo-pile it will produce a current of *electricity;* if I throw it upon a sensitive photographic plate it will produce *chemical action;* and if I throw it upon the instrument I have just described it will produce *motion.* What, then, am I to call that ray? Is it light, heat, electricity, chemical action, or motion? It is neither. All these actions are inseparable attributes of the ray of that particular wave-length, and are not evidences of separate identities. I can no more split that ray up into five or six different rays, each having different properties, than I can split up the element iron, for instance, into other elements, one possessing the specific gravity of iron, another its magnetic properties, a third its chemical properties, a fourth its conducting power for heat, and so on. A ray of light of a definite refrangibility is one and indivisible, just as an element is, and these different properties of the ray are mere functions of that refrangibility, and inseparable from it. Therefore when I tell you that a ray in the ultra red pushes the instrument with a force of 100, and a ray in the most luminous part has a dynamic value of about half that, it must be understood that the latter action is not due to heat-rays which accompany the luminous rays, but that the action is one purely due to the wave-length and the refrangibility of the ray employed. You now understand why it is that I cannot give a definite answer to the question—"Is it heat or is it light that produces these movements?" There is no physical difference between heat and light, so, to avoid confusion, I call the total bundle of rays which come from a candle or the sun, *radiation.*

I found, by throwing the pure rays of the spectrum one after the other upon this apparatus, that I could obtain a very definite answer to my first question—"What are the actual rays which cause this action?"

The apparatus was fitted up in a room specially devoted to it, and was protected on all sides, except where the rays

of light had to pass, with cotton-wool and large bottles of water. A heliostat reflected a beam of sunlight in a constant direction, and it was received on an appropriate arrangement of slit, lenses, prisms, &c., for projecting a pure spectrum. Results were obtained in the months of July, August, and September; and they are given in the figure (Fig. 5) graphically as a curve, the maximum being in the ultra-red and the minimum in the ultra-violet. Taking the maximum at 100, the following are the mechanical values of the different colours of the spectrum:—

Ultra-red	100
Extreme red	85
Red	73
Orange	66
Yellow	57
Green	41
Blue	22
Indigo	8½
Violet	6
Ultra-violet	5

A comparison of these figures is a sufficient proof that the mechanical action of radiation is as much a function of the luminous rays as it is of the dark heat-rays.

The second question, namely, "What influence has the colour of the surface on the action?" has also been solved by this apparatus.

In order to obtain comparative results between discs of pith coated with lamp-black and with other substances, another torsion apparatus was constructed, in which six discs *in vacuo* could be exposed one after the other to a standard light. One disc always being lamp-blacked pith, the other discs could be changed so as to get comparisons of action. Calling the action of radiation from a candle on the lamp-blacked disc 100, the following are the proportions obtained:—

Lamp-blacked pith	100
Iodide of palladium	87·3
Precipitated silver	56
Amorphous phosphorus	40
Sulphate of baryta	37
Milk of sulphur	31
Red oxide of iron	28
Scarlet iodide of mercury and copper	22
Lamp-blacked silver	18
White pith	18
Carbonate of lead	13
Rock-salt	6·5
Glass	6·5

This table gives important information on many points: one more especially—the action of radiation on lamp-

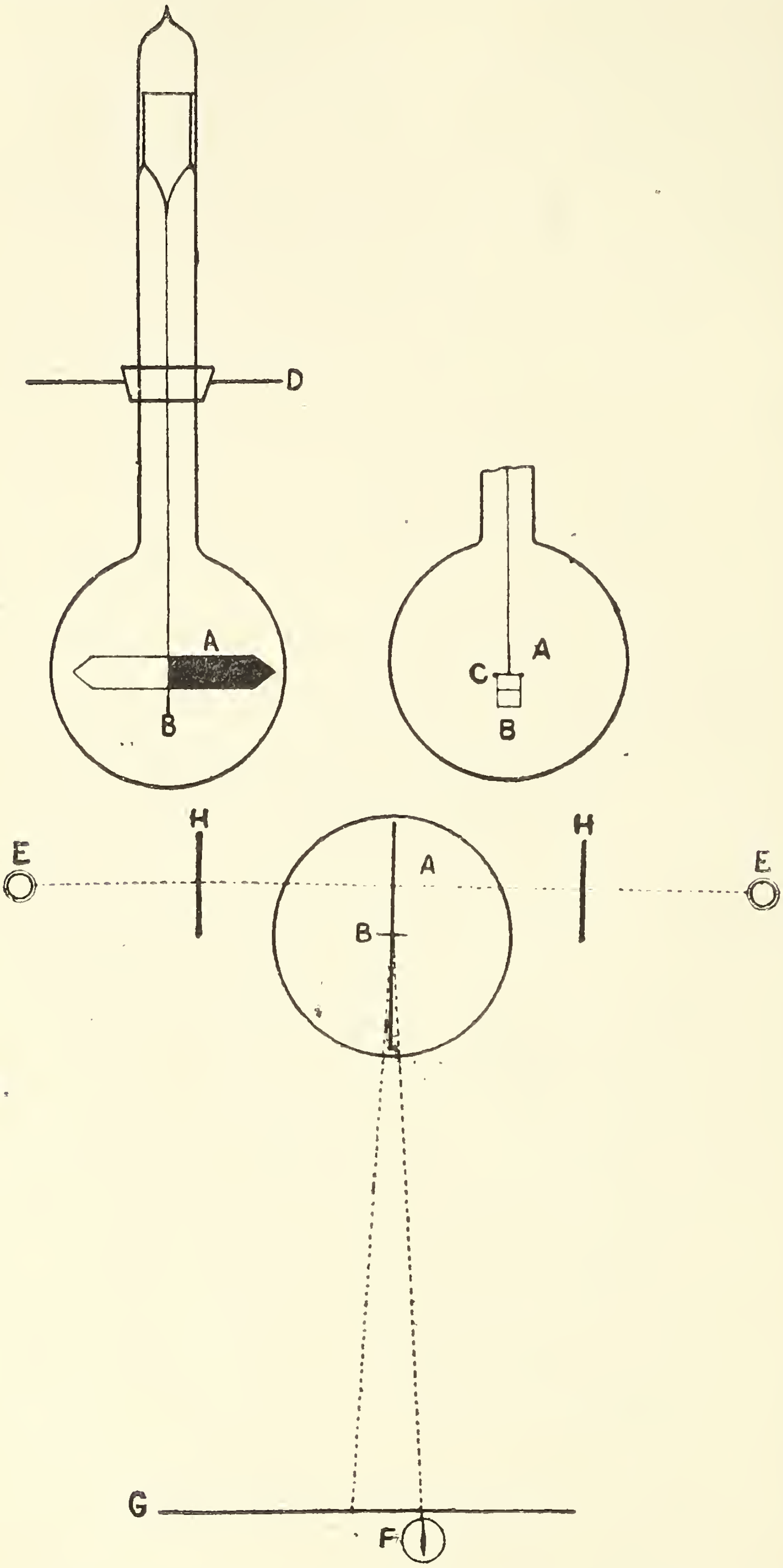


FIG. 6.

blacked pith is $5\frac{1}{2}$ times what it is on plain pith. A bar like those used in my first experiment, having one-half

black and one-half white, exposed to a broad beam of radiation, will be pushed with $5\frac{1}{2}$ times more strength on the black than on the white half, and if freely suspended will set at an angle greater or less according to the intensity of the radiation falling on it.

This suggests the employment of such a bar as a photometer, and I have accordingly made an instrument on this principle: its construction is shown in the diagram (Fig. 6). It consists of a flat bar of pith, A, half black and half white, suspended horizontally in a bulb by means of a long silk fibre. A reflecting mirror, B, and small magnet, C, are fastened to the pith, and a controlling magnet, D, is fastened outside so that it can slide up and down the tube, and thus increase or diminish sensitiveness. The whole is completely exhausted and then enclosed in a box lined with black velvet, with apertures for the rays of light to pass in and out. A ray of light from a lamp, F, reflected from the mirror, B, to a graduated scale, G, shows the movements of the pith bar.

The instrument fitted up for a photometric experiment is in front of me on the table. A beam from the electric light falls on the little mirror, and is thence reflected back to the screen, where it forms a spot of light, the displacement of which to the right or the left shows the movement of the pith bar. One end of the bar is blacked on each side, the other end being left plain. I have two candles, E E, each 12 inches off the pith bar, one on each side of it. When I remove the screens, H H, the candle on one side will give the pith a push in one direction, and the candle on the other side will give the pith a push in the opposite direction, and as they are the same distance off they will neutralise each other, and the spot of light will not move. I now take the two screens away: each candle is pushing the pith equally in opposite directions, and the luminous index remains at zero. When, however, I cut one candle off, the candle on the opposite side exerts its full influence, and the index flies to one end of the scale. I cut the other one off and obscure the first, and the spot of light flies to the other side. I obscure them both, and the index comes quickly to zero. I remove the screens simultaneously, and the index does not move.

I will retain one candle 12 inches off, and put two candles on the other side 17 inches off. On removing the screens you see the index does not move from zero. Now the square of 12 is 144, and the square of 17 is 289. Twice 144 is 288. The light of these candles, therefore,

is as 288 to 289. They therefore balance each other as nearly as possible. Similarly I can balance a gas-light against a candle. I have a small gas-burner here, which I place 28 inches off on one side, and you see it balances the candle 12 inches off. These experiments show how conveniently and accurately this instrument can be used as a photometer. By balancing a standard candle on one side against any source of light on the other, the value of the latter in terms of a candle is readily shown; thus in the last experiment the standard candle 12 inches off is balanced by a gas-flame 28 inches off. The lights are therefore in the proportion of 12^2 to 28^2, or as 1 to 5·4. The gas-burner is therefore equal to about $5\frac{1}{2}$ candles.

In practical work on photometry it is often required to ascertain the value of gas. Gas is spoken of commercially as of so many candle-power. There is a certain "standard" candle which is supposed to be made invariable by Act of Parliament. I have worked a great deal with these standard candles, and I find them to be among the most variable things in the world. They never burn with the same luminosity from one hour to the other, and no two candles are alike. I can now, however, easily get over this difficulty. I place a "standard" candle at such a distance from the apparatus that it gives a deflection of 100 degrees on the scale. If it is poorer than the standard, I bring it nearer; if better, I put it farther off. Indeed any candle may be taken; and if it be placed at such a distance from the apparatus that it will give a uniform deflection, say of 100 divisions, the standard can be reproduced at any subsequent time; and the burning of the candle may be tested during the photometric experiments by taking the deflection it causes from time to time, and altering its distance, if needed, to keep the deflection at 100 divisions. The gas-light to be tested is placed at such a distance on the opposite side of the pith bar that it exactly balances the candle. Then, by squaring the distances, I get the exact proportion between the gas and the candle.

Before this instrument can be used as a photometer or light measurer, means must be taken to cut off from it all those rays coming from the candle or gas which are not actually luminous. A reference to the spectrum diagram (Fig. 5) will show that at each end of the coloured rays, there is a large space inactive, as far as the eye is concerned, but active in respect to the production of motion—strongly so at the red end, less strong at the violet end. Before the instrument can be used to measure luminosity, these rays

must be cut off. We buy gas for the light that it gives, not for the heat it evolves on burning, and it would therefore never do to measure the heat and pay for it as light.

It has been found that a clear plate of alum, whilst letting all the light through, is almost, if not quite, opaque to the heating rays below the red. A solution of alum in water is almost as effective as a crystal of alum; if, therefore, I place in front of the instrument glass cells containing an aqueous solution of alum, the dark heat rays are filtered off.

But the ultra-violet rays still pass through, and to cut these off I dissolve in the alum solution a quantity of sulphate of quinine. This body has the property of cutting off the ultra-violet rays from a point between the lines G and H. A combination of alum and suphate of quinine, therefore, limits the action to those rays which affect the human eye, and the instrument, such as you see it before you, becomes a true photometer.

This instrument, when its sensitiveness is not deadened by the powerful control magnet I am obliged to keep near it for these experiments, is wonderfully sensible to light. In my own laboratory a candle 36 feet off produces a decided movement, and the motion of the index increases inversely with the square of the distance, thus answering the third question—"Is the amount of action in direct proportion to the amount of radiation?"

The experimental observations and the numbers which are required by the theoretical diminution of light with the square of the distance, are sufficiently close, as the following figures show:—

Candle	6 feet off	gives a	deflection of	218·0°
„	12	„	„	54·0°
„	18	„	„	24·5°
„	24	„	„	13·0°
„	10	„	„	77·0°
„	20	„	„	19·0°
„	30	„	„	8·5°

The effect of two candles side by side is practically double, and of three candles three times that of one candle.

In the instrument just described the candle acts on a pith bar, one end of which is blacked on each side. But suppose I black the bar on alternate halves and place a light near it sufficiently strong to drive the bar half round. The light will now have presented to it another black surface in the same position as the first, and the bar will be again driven

in the same direction half round. This action will be again repeated, the differential action of the light on the black and white surfaces keeps the bar moving, and the result will be rotation.

Here is such a pith bar, blacked on alternate sides, and suspended in an exhausted glass bulb (Fig. 7). I project its

FIG. 7.

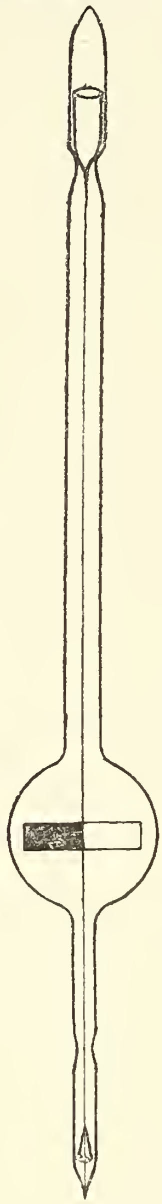
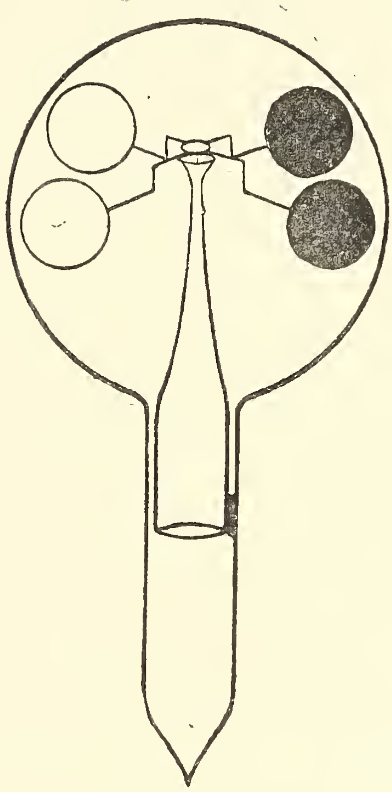


image on the screen, and the strong light which shines on it sets it rotating with considerable velocity. Now it is slackening speed, and now it has stopped altogether. The bar is supported on a fibre of silk, which has twisted round till the rotation is stopped by the accumulated torsion. I put a water screen between the bar and the electric light to cut off some of the active rays, and the silk untwists, turning

the bar in the opposite direction. I now remove the water, and the bar revolves rapidly as at first.

From suspending the pith on a silk fibre to balancing it on a point the transition is slight; the interfering action of torsion is thereby removed, and the instrument rotates continuously under the influence of radiation. Many of these little pieces of apparatus, to which I have given the name of radiometers, are on the table, revolving with more or less speed. The diagram (Fig. 8) shows their construction, which is very simple. They are formed of four arms of very fine glass, supported in the centre by a needle-point, and having

FIG. 8.



at the extremities thin discs of pith lampblacked on one side, the black surfaces all facing the same way. The needle stands in a glass cup, and the arms and discs are delicately balanced so as to revolve with the slightest impetus.

Here are some rotating by the light of a candle. This one is now rather an historical instrument, being the first one in which I saw rotation. It goes very slowly in comparison with the others, but it is not bad for the first instrument of the sort that was ever made.

I will now, by means of a vertical lantern, throw on the screen the projection of one of these instruments, so as to show the movement rather better than you could see it on

the table. The electric light falling vertically downwards on it, and much of the power being cut off by water and alum screens, the rotation is slow. I bring a candle near and the speed increases. I now lift the radiometer up, and place it full in the electric light, projecting its image direct on the screen, and it goes so rapidly that if I had not cut out the four pieces of pith of different shapes you would have been unable to follow the movement.

The speed with which a sensitive radiometer will revolve in the sun is almost incredible; and the electric light such as I have it in this lantern cannot be far short of full sunshine. Here is the most sensitive instrument I have yet made, and I project its image on the screen, letting the full blaze of the electric light shine upon it. Nothing is seen but an undefined nebulous ring, which becomes at times almost invisible. The number of revolutions per second cannot be counted, but they must be several hundreds, for one candle has made it spin round forty times a second.

I have called the instrument the radiometer because it will enable me to measure the intensity of radiation falling on it by counting the revolutions in a given time; the law being that the rapidity of revolution is inversely as the square of the distance between the light and the instrument.

When exposed to different numbers of candles at the same distance off, the speed of revolution in a given time is in proportion to the number of candles; two candles giving twice the rapidity of one candle, and three, three times, &c.

The position of the light in the horizontal plane of the instrument is of no consequence, provided the distance is not altered; thus two candles, one foot off, give the same number of revolutions per second, whether they are side by side or opposite to each other. From this it follows that if the radiometer is brought into a uniformly lighted space it will continue to revolve.

It is easy to get rotation in a radiometer without having the surfaces of the discs differently coloured. Here is one having the pith discs blacked on both sides. I project its image on the screen, and there is no movement. I bring a candle near it, and shade the light from one side, when rapid rotation is produced, which is at once altered in direction by moving the shade to the other side.

I have arranged here a radiometer so that it can be made to move by a very faint light, and at the same time its rotation is easily followed by all present. In this bulb is a large six-armed radiometer carrying a mirror in its centre. The mirror is almost horizontal, but not quite so, and there-

fore when I throw a beam of electric light vertically downwards on to the central mirror, the light is reflected off at a slight angle, and as the instrument rotates its movement is shown by the spot of light travelling round the ceiling in a circle. Here again the fog helps us, for it gives us an imponderable beam of light moving round the room like a solid body, and saving you the trouble of looking up to the ceiling. I now set the radiometer moving round by the light of a candle, and I want to show you that coloured light does not very much interfere with the movement. I place yellow glass in front, and the movement is scarcely diminished at all. Very deep coloured glass, you see, diminishes it a little more. Blue and green glass make it go a little slower, but still do not diminish the speed one-half. I now place a screen of water in front: the instrument moves with diminished velocity, rotating with about one-fourth its original speed.

Taking the action produced by a candle flame as 100

Yellow	glass	reduces	it to . . .	89
Red	,,	,,	,, . . .	71
Blue	,,	,,	,, . . .	56
Green	,,	,,	,, . . .	56
Water		,,	,, . . .	26
Alum		,,	,, . . .	15

I now move the candle a little distance off, so as to make the instrument move slower, and bring a flask of boiling water close to it. See what happens. The luminous index no longer moves steadily, but in jerks. Each disc appears to come up to the boiling water with difficulty, and to hurry past it. More and more sluggishly do they move past, until now one has failed to get by, and the luminous beam, after oscillating to and fro a few times, comes to rest. I now gradually bring the candle near. The index shows no movement. Nearer still. There is now a commencement of motion, as if the radiometer was trying to push past the resistance offered by the hot water; but it is not until I have brought the candle to within a few inches of the glass globe that rotation is recommenced. On these pith radiometers the action of dark heat is to repel the black and white surfaces almost equally, and this repulsion is so energetic as to overcome the rotation caused by the candle, and to stop the instrument.

With a radiometer constructed of a good conductor of heat, such as metal, the action of dark heat is different. Here is one made of silvered copper, polished on one side and lampblacked on the other. I have set it moving with a candle slightly the normal way. Here is a glass shade heated

so that it feels decidedly warm to the hand. I cover the radiometer with it, and the rotation first stops, and then recommences the reverse way. On removing the hot shade the reverse movement ceases, and normal rotation recommences.

If, however, I place a hot glass shade over a pith radiometer the arms at once revolve the normal way, as if I had exposed the instrument to light. The diametrically opposite behaviour of a pith and a metal instrument when exposed to the dark heat radiated from a hot glass shade is very striking. The explanation of the action is not easy, but it depends on the fact that the metal is one of the best conductors of heat, whilst pith is one of the worst.

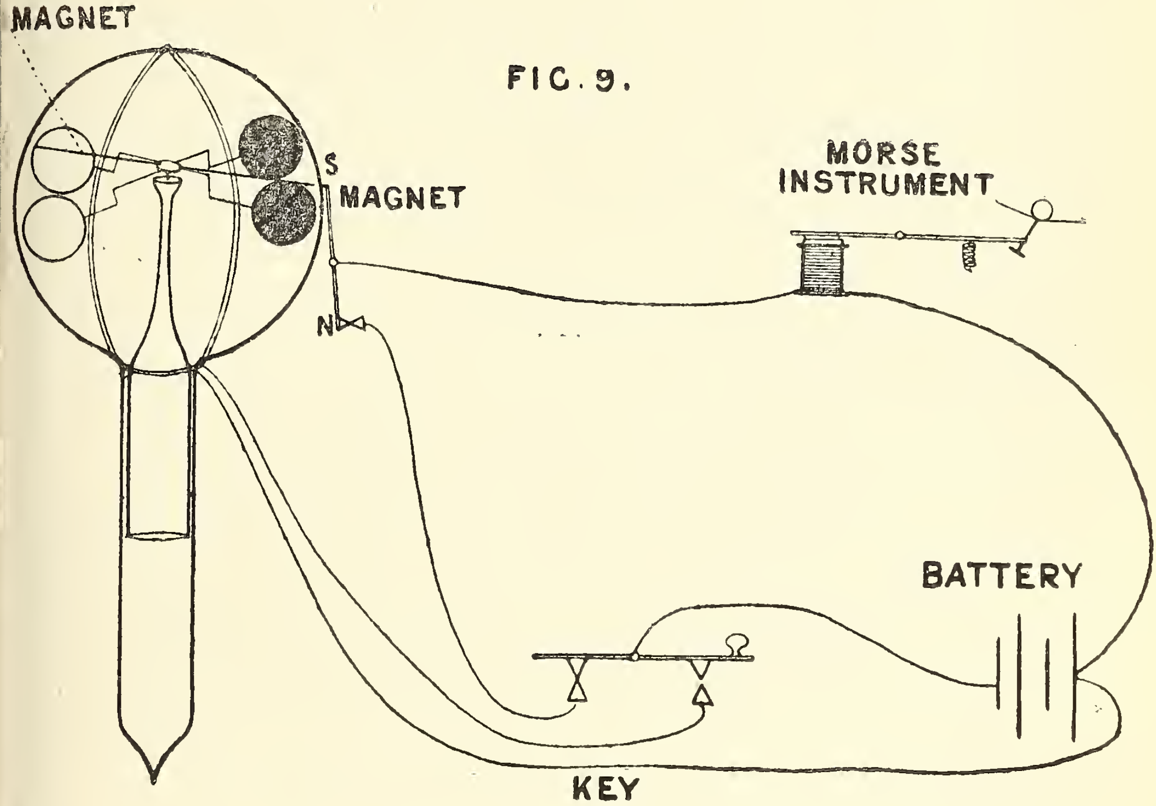
One more experiment with this metallic radiometer. I heat it strongly with a spirit-lamp, and the arms spin round rapidly. Now the whole bulb is hot, and I remove the lamp: see what happens. The rotation quickly diminishes. Now it is at rest; and now it is spinning round just as fast the reverse way. I can produce this reverse movement only with difficulty with a pith instrument. The action is due to the metal being a good conductor of heat. As it absorbs heat it moves one way; as it radiates heat it moves the opposite way.

At first I made these instruments of the very lightest material possible, some of them not weighing more than half a grain; and where extreme sensitiveness is required lightness is essential. But the force which carries them round is quite strong enough to move a much greater weight. Thus the metallic instrument I have just experimented with weighs over 13 grains, and here is one still heavier, made of four pieces of looking-glass blacked on the silvered side, which are quickly sent round by the impact of this imponderable agent, and flash the rays of light all round the room when the electric lamp is turned on the instrument.

Before dismissing this instrument let me show one more experiment. I place the looking-glass and the metal radiometer side by side, and, screening the light from them, they come almost to rest. Their temperature is the same as that of the room. What will happen if I suddenly chill them? I pour a few drops of ether on each of the bulbs. Both instruments begin to revolve. But notice the difference. Whilst the movement in the case of the metal radiometer is direct, that of the looking-glass instrument is reverse. And yet to a candle they both rotate the same way, the black being repelled.

Now, having found that this force would carry round a comparatively heavy weight, another useful application

suggested itself. If I can carry round heavy mirrors or plates of copper, I can carry round a magnet. Here, then (Fig. 9), is an instrument carrying a magnet, and outside is a smaller magnet, delicately balanced in a vertical position, having the south pole at the top and the north pole at the bottom. As the inside magnet comes round, the outside magnet, being delicately suspended on its centre, bows backwards and forwards, and, making contact at the bottom, carries an electric current from a battery to a Morse instrument. A ribbon of paper is drawn through the "Morse" by clockwork, and at each contact—at each revolution of the radiometer—a record



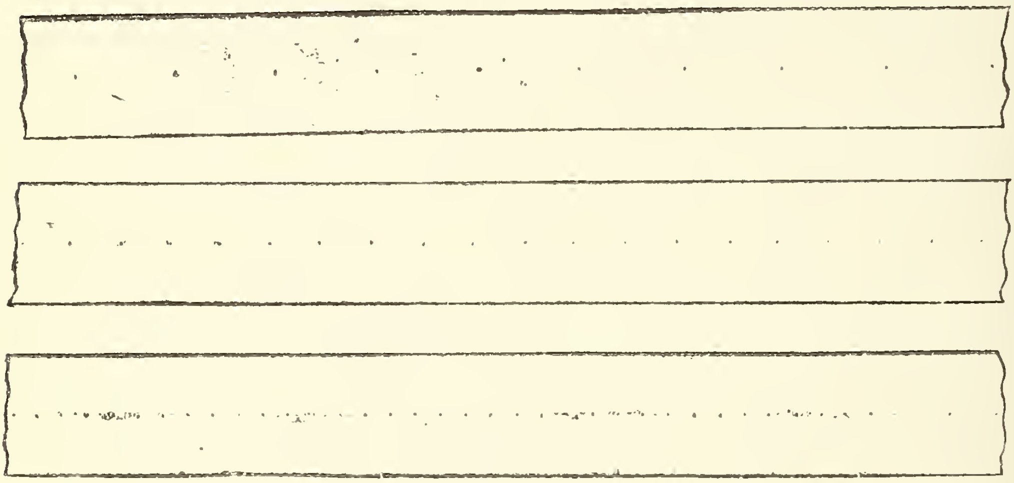
is printed on the strip of paper by dots; close together if the radiometer revolves quickly, farther apart if it goes slower.

Here the inner magnet is too strong to allow the radiometer to start with a faint light without some initial impetus. Imagine the instrument to be on the top of a mountain away from everybody, and I wish to start it in the morning. Outside the bulb are a few coils of insulated copper wire, and by depressing the key for an instant I pass an electric current from the battery through them. The interior magnet is immediately deflected from its north-south position, and the impetus thus gained enables the light to keep up the rotation. In a proper meteorological instrument I should

have an astatic combination inside the bulb, so that a very faint light would be sufficient to start it, but in this case I am obliged to set it going by an electric current. I have placed a candle near the magnetic radiometer. I now touch the key; the instrument immediately responds; the paper unwinds from the Morse instrument, and on it you will see dots in regular order. I put the candle 8 inches off, and the dots come wide apart. I place it $5\frac{3}{4}$ inches off, and two dots come where one did before. I bring the candle 4 inches from the instrument, and the dots become four times as numerous (Fig. 10), thus recording automatically the intensity of the light falling on the instrument, and proving that in this case also the radiometer obeys the law of inverse squares.

This instrument, the principle of which I have illustrated to-night, is not a mere toy or scientific curiosity, but it is capable of giving much useful information in climatology. You are well aware that the temperature, the rainfall, the atmospheric pressure, the direction and force of the wind,

FIG. 10.



are now carefully studied in most countries, in order to elucidate their sanitary condition, their animal and vegetable productions, and their agricultural capabilities. But one most important element, the amount of light received at any given place, has been hitherto but very crudely and approximately estimated, or rather guessed at. Yet it cannot be denied that sunlight has its effect upon life and health, vegetable, animal, and human, and that its relative amount at any place is hence a point of no small moment. The difficulty is now overcome by such an instrument as this. The radiometer may be permanently placed on some tall building, or high mountain, and, by connecting it by

telegraphic wires to a central observatory, an exact account can be kept of the proportion of sunlight received in different latitudes, and at various heights above the sea-level. Furthermore, our records of the comparative temperature of different places have been hitherto deficient. The temperature of a country depends partly on the amount of rays which it receives direct from the sun, and partly on the atmospheric and oceanic currents, warm or cold, which sweep over or near it. The thermometer does not discriminate between these influences; but the radiometer will enable us now to distinguish how much of the annual temperature of a place is due to the direct influence of the sun alone, and how much to the other factors above referred to.

I now come to the last question which I stated at the beginning of this lecture—"What is the amount of force exerted by radiation?" Well, I can calculate out the force in a certain way, from data supplied by this torsion apparatus (Fig. 4). Knowing the weight of the beam, the power of the torsion fibre of glass, its time of oscillation, and the size of the surface acted on, it is not difficult to calculate the amount of force required to deflect the beam through a given angle; but I want to get a more direct measure of the force. I throw a ray of light upon one of these instruments, and it gives a push; surely it is possible to measure the amount of this push in parts of a grain. This I have succeeded in doing in the instrument behind me; but before showing the experiment I want to illustrate the principle upon which it depends. Here is a very fine glass fibre suspended from a horizontal bar, and I wish to show you the strength of it. The fibre is only a few thousandths of an inch thick; it is about 3 feet long, and at the lower end is hanging a scale-pan, weighing 100 grains. So I start with a pull of 100 grains on it. I now add little lead weights, 50 grains each, till it breaks. It bears a pull of 750 grains, but gives way when additional weight is added. You see then the great strength of a fibre of glass, so fine as to be invisible to all who are not close to it, to resist a tensile strain.

Now I will illustrate another equally important property of a glass thread, viz., its power to resist torsion. Here is a still finer glass thread, stretched horizontally between two supports; and in order to show its position I have put little jockeys of paper on it. One end is cemented firmly to a wooden block, and the other end is attached to a little instrument called a counter—a little machine for registering

the number of revolutions. I now turn this handle till the fibre breaks, and the counter will tell me how many twists I have given this fibre of glass. You see it breaks at twenty revolutions. This is rather a thicker fibre than usual. I have had them bear more than 200 turns without breaking, and some that I have worked with are so fine that if I hold one of them by the end it curls itself up and floats about the room like a piece of spider's thread.

Having now illustrated these properties of glass fibres, I will try to show a very delicate experiment. I want to ascertain the amount of pressure which radiation exerts on a blackened surface. I will put a ray of light on the pan of a balance, and give you its weight in grains, for I think in this Institution and before this audience I may be allowed a scientific use of the imagination, and may speak of weighing that which is not affected by gravitation.

The principle of the instrument is that of W. Ritchie's torsion balance, described by him in the "Philosophical Transactions" for 1830. The construction is somewhat complicated, but it can be made out on reference to the diagram (Fig. 11). A light beam, A B, having 2 square inches of pith, C, at one end, is balanced on a very fine fibre of glass, D D′, stretched horizontally in a tube; one end of the fibre being connected with a torsion handle, E, passing through the tube, and indicating angular movements on a graduated circle. The beam is cemented to the torsion fibre, and the whole is enclosed in glass and connected with the mercury pump by a spiral tube, F, and exhausted as perfectly as possible. G is a spiral spring, to keep the fibre in a uniform state of tension. H is a piece of cocoon silk. I is a glass stopper, which is ground into the tube as perfectly as possible, and then highly polished and lubricated with melted india-rubber, which is the only substance I know that allows perfect lubrication and will still hold a vacuum. The pith, C, represents the scale-pan of the balance. The cross-beam, A B, which carries it, is cemented firmly to the thin glass fibre, D, and in the centre is a piece of mirror, K. Now the cross-beam A B and the fibre D being rigidly connected together, any twist which I give to the torsion handle E will throw the beam out of adjustment. If, on the other hand, I place a weight on the piece of pith C, that end of the beam will fall down, and I shall have to turn the handle, E, round and round a certain number of times, until I have put sufficient torsion on the fibre D to lift up the beam. Now, according to the law of

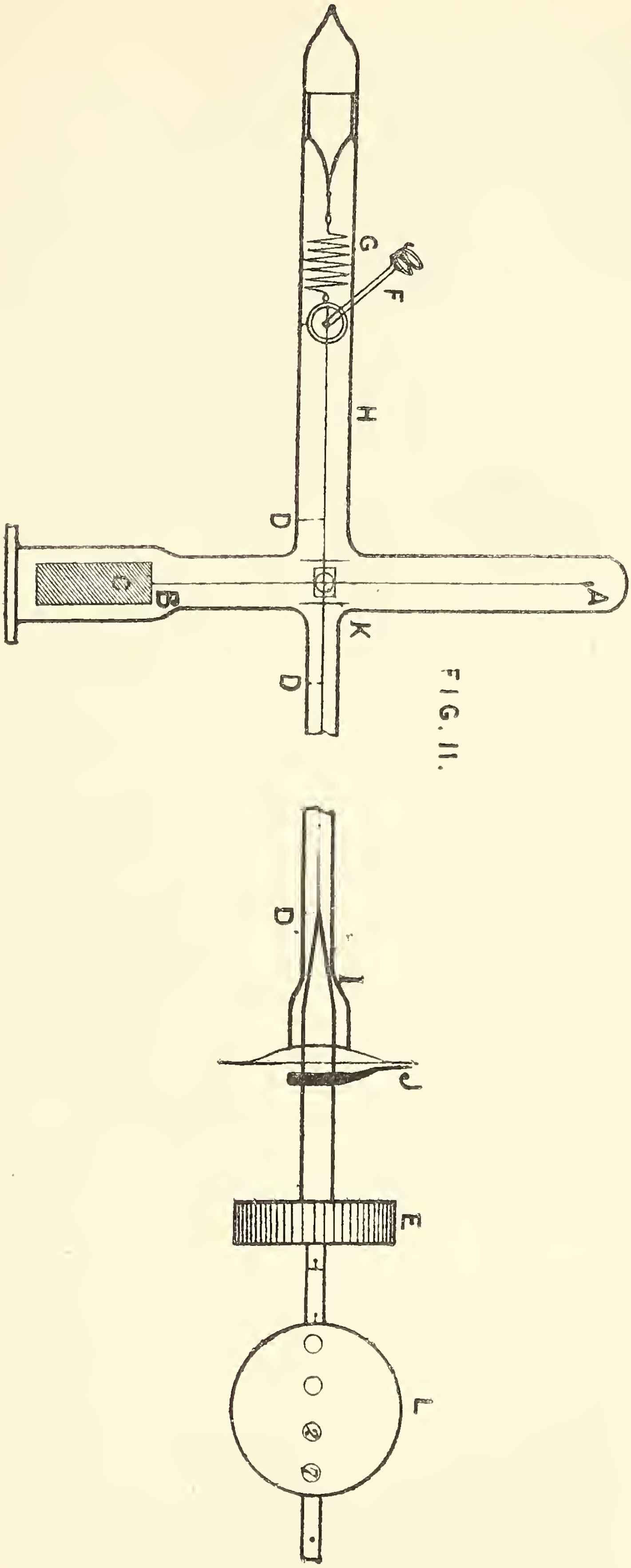


FIG. II.

torsion, the force with which a perfectly elastic body like glass tends to untwist itself is directly proportional to the number of degrees through which it has been twisted; therefore, knowing how many degrees of torsion I must put on the fibre to lift up the 1-100th of a grain weight, I can tell how many degrees of torsion are required to lift up any other weight; and conversely, putting an unknown weight or pressure on the pith, I can find its equivalent in grains by seeing how much torsion it is equal to. Thus, if 1-100th of a grain requires 10,000 degrees of torsion, 1-50th of a grain would require 20,000 degrees; and conversely, a weight which required 5000 degrees torsion would weigh 1-200th of a grain. Once knowing the torsion equivalent of 1-100th of a grain, the ratio of the known to the unknown weights is given by the degrees of torsion.

Having thus explained the working of the torsion balance I will proceed to the actual experiment. On the central mirror I throw a ray from the electric light, and the beam reflected on a particular spot of the ceiling will represent zero. The graduated circle J of the instrument also stands at zero, and the counter which I fasten on at the end L stands at 0. The position of the spot of light reflected from the little concave mirror being noted, the torsion balance enables me to estimate the pressure or weight of a beam of light to a surprising degree of exactness. I lift up my little iron weight by means of a magnet (for working in a vacuum I am restricted in the means of manipulating), and drop it in the centre of the pith: it knocks the scale-pan down, as if I had placed a pound weight upon an ordinary balance, and the index ray of light has flown far from the zero-point on the ceiling. I now put torsion on the fibre to bring the beam again into equilibrium. The index-ray is moving slowly back again. At last it is at zero, and on looking at the circle and counter I see that I have had to make 27 complete revolutions and 301 degrees, or $27 \times 360^\circ + 301^\circ = 10{,}021^\circ$, before the force of torsion would balance the 1-100th of a grain.

I now remove the weight from the pith-pan of my balance, and liberate the glass thread from torsion by twisting it back again. Now the spot of light on the ceiling is at zero, and the counter and index are again at 0.

Having thus obtained the value of the 1-100th of a grain in torsion degrees, I will get the same for the radiation from a candle. I place a lighted candle exactly 6 inches from the blackened surface, and on removing the screen the pith scale-pan falls down, and the index-ray again flies across the

ceiling. I now turn the torsion handle, and in much less time than in the former case the ray is brought back to zero. On looking at the counter I find it registers four revolutions, and the index points to 188 degrees, making altogether $360^\circ \times 4 + 188 = 1628^\circ$, through which the torsion fibre has to be twisted to balance the light of the candle.

It is an easy calculation to convert this into parts of a grain weight; 10,021 torsion degrees representing 0·01 grain, 1628 torsion degrees represent 0·001624 grain.

$$10{,}021^\circ : 0{\cdot}01 \text{ grain} :: 1628^\circ : 0{\cdot}001624 \text{ grain}.$$

The radiation of a candle 6 inches off, therefore, weighs or presses the two square inches of blackened pith with a weight of 0·001624 grain. In my own laboratory, working with this torsion balance, I found that a candle 6 inches off gave a pressure of 0·001772 grain. The difference is only 0·000148 grain, and is fairly within the allowable limits of a lecture experiment. But this balance is capable of weighing to far greater accuracy than that. You have seen that a torsion of 10,021° balanced the hundredth of a grain. If I give the fibre 1 degree more twist the weight is over-balanced, as shown by the movement of the index-ray on the ceiling. Now 1 degree of torsion is about the 1-10,000th part of the whole torsion required by the 1-100th grain. It represents therefore the 1-10,000th part of the 1-100th, or the millionth part of a grain.

Divide a grain weight into a million parts, place one of them on the pan of the balance, and the beam will be instantly depressed!

Weighed in this balance the mechanical force of a candle 12 inches off was found to be 0·000444 grain; of a candle 6 inches off, 0·001772 grain. At half the distance the weight of radiation should be four times, or 0·001776 grain; the difference between theory and experiment being only four-millionths of a grain is a sufficient proof that the indications of this instrument, like those of the apparatus previously described, follow the law of inverse squares. An examination of the differences between the separate observations and the mean shows that my estimate of the sensitiveness of this balance is not excessive, and that in practice it will safely indicate the millionth of a grain.

I have only had one opportunity of getting an observation of the weight of sunlight: it was taken on December 13th, but the sun was so obscured by thin clouds and haze that it was only equal to 10·2 candles 6 inches off. Calculating from

this datum, it is seen that the pressure of sunshine is 2·3 tons per square mile.

But however fair an equivalent ten candles may be for a London sun in December, a midsummer sun in a cloudless sky has a very different value. Authorities differ as to its exact equivalent, but I under-estimate it at 1000 candles 12 inches off.

Let us see what pressure this will give:—A candle 12 inches off, acting on 2 square inches of surface, was found equal to 0·000444 grain; the sun, equalling 1000 candles, therefore gives a pressure of 0·444000 grain; that is equal to about 32 grains per square foot, to 2 cwts. per acre, 57 tons per square mile, or nearly three thousand million tons on the exposed surface of the globe—sufficient to knock the earth out of its orbit if it came upon it suddenly.

It may be said that a force like this must alter our ordinary ideas of gravitation; but it must be remembered that we only know the force of gravity as between bodies such as they actually exist, and we do not know what this force would be if the temperatures of the gravitating masses were to undergo a change. If the sun is gradually cooling, possibly its attractive force is increasing, but the rate will be so slow that it will probably not be detected by our present means of research.

Whilst showing this experiment I wish to have it distinctly understood that I do not attach the least importance to the actual numerical results. I simply wish to show you the marvellous sensitiveness of the apparatus with which I am accustomed to work. I may, indeed, say that I know these rough estimates to be incorrect. It must be remembered that our earth is not a lamp-blacked body enclosed in a glass case, nor is its shape such as to give the maximum of surface with the minimum of weight. The solar forces which perpetually pour on it are not simply absorbed and degraded into radiant heat, but are transformed into the various forms of motion we see around us, and into the countless forms of vegetal, animal, and human activity. The earth, it is true, is poised in vacuous space, but it is surrounded by a cushion of air; and, knowing how strongly a little air stops the movement of repulsion, it is easy to conceive that the sun's radiation through this atmospheric layer may not produce any important amount of repulsion. It is true the upper surface of our atmosphere must present a very cold front, and this might suffer repulsion by the sun; but I have said enough to show how utterly in the dark we are as to the cosmical bearings of this action

of radiation, and further speculation would be but waste of time.

It may be of interest to compare these experimental results with a calculation made in 1873, before any knowledge of these facts had been made public.

Prof. Clerk Maxwell, in his "Electricity and Magnetism," vol. ii., p. 391, writes as follows:—"The mean energy in one cubic foot of sunlight is about 0·0000000882 of a foot-pound, and the mean pressure on a square foot is 0·0000000882 of a pound weight. A flat body exposed to sunlight would experience this pressure on its illuminated side only, and would therefore be repelled from the side on which the light falls."

Calculated out, this gives the pressure of sunlight equal to about $2\frac{1}{2}$ lbs. per square mile. Between the $2\frac{1}{2}$ lbs. deduced from calculation and the 57 tons obtained from experiment the difference is great; but not greater than is often the case between theory and experiment.

In conclusion, I beg to call especial attention to one not unimportant lesson which may be gathered from this discovery. It will be at once seen that the whole springs from the investigation of an anomaly. Such a result is by no means singular. Anomalies may be regarded as the finger-posts along the high road of research, pointing to the bye-ways which lead to further discoveries. As scientific men are well aware, our way of accounting for any given phenomenon is not always perfect. Some point is perhaps taken for granted, some peculiar circumstance is overlooked. Or else our explanation agrees with the facts not perfectly, but merely in an approximate manner, leaving a something still to be accounted for. Now these residual phenomena, these very anomalies, may become the guides to new and important revelations.

In the course of my research anomalies have sprung up in every direction. I have felt like a traveller navigating some mighty river in an unexplored continent. I have seen to the right and the left other channels opening out, all claiming investigation, and promising rich rewards of discovery for the explorer who shall trace them to their source. Time has not allowed me to undertake the whole of a task so vast and so manifold. I have felt compelled to follow out, as far as lay in my power, my original idea, passing over reluctantly the collateral questions springing up on either hand. To these I must now invite the attention of my fellow-workers in Science. There is ample room for many enquirers.

Nor must we forget that the more rigidly we scrutinise our received theories, our routine explanations, and interpretations of Nature, and the more frankly we admit their shortcomings, the greater will be our ultimate reward. In the practical world fortunes have been realised from the careful examination of what has been ignorantly thrown aside as refuse; no less, in the sphere of Science, are reputations to be made by the patient investigation of anomalies.

NOTICES OF BOOKS.

Christian Psychology ; the Soul and the Body in their Correlation and Contrast. Being a New Translation of Swedenborg's Tractate "*De Commercio Animi et Corporis.*" With Preface and Illustrative Notes. By T. M. Gorman, M.A. London: Longmans and Co.

It is a subject which might be debated at great length in how far the present work can legitimately come under our cognisance. The translator, indeed, assures us in his preface that the contents of the original work of Swedenborg are "of a strictly philosophical character," but he immediately adds "although it evidently contains several distinct indications of having been written with a definite theological aim." The work before us, however, to 113 pages due to Swedenborg himself, contains upwards of 400 for which Mr. Gorman must be held responsible, aad in which the theological element and the theological style are still more pronounced. In the preface is a vigorous attack upon a certain Vicar of Frome-Selwood, and a still fiercer onslaught upon Cardinal Manning. There is a protest warning the world not in any way to confound Swedenborg with the Swedenborgians, who are described as "the small sect founded in the year 1787, under most unhappy auspices, by a certain Robert Hindmarsh, of Clerkenwell Close, a layman, and formerly a member of Mr. Wesley's Communion." In all this we can see very little of philosophical interest.

The main purpose of the Appendix is declared to be "to furnish further illustration of the text, and to indicate a few relations of agreement or opposition which appear to exist between certain speculations which have obtained considerable currency in the present day, and some of the more important principles taught in our author's writings more than a century ago. This may, perhaps, be a fitting place to attempt the difficult task of furnishing an estimate of Swedenborg's character as a man of Science. Years ago we took up certain of his writings—especially a treatise on chemistry—with a very strong prepossession, not against him, but in his favour. We had heard it declared that Swedenborg had all but anticipated the Atomic Theory, and that his works teemed with valuable suggestions. We read accordingly, and re-read, eagerly and carefully. But the more we read the more we were disappointed. What we sought was not there. There was nothing to point the path to new regions of discovery; there were no ideas, at least to our comprehension, which admitted of experimental verification. Reluctantly we

laid the book aside, doubting whether the fault lay with the author or with ourself. But when we came upon the "Philosophie Chimique" of Dumas we found his estimate of Swedenborg as a chemist agree very closely with our own. Since that time we have watched the gradual development of chemical science; we have taken part in the attack and defence of theories and systems, but we have not succeeded in detecting, in the course of modern discovery, anything which is fairly due to the inspiration of Swedenborg, or of which his writings can be considered the fountain. At the same time we do not dispute that a subtle and eager partisan might imagine that he saw in the fruits of modern research a fulfilment of some of the foreshadowings of this remarkable man, and, from the peculiar nature of Swedenborg's writings, it might be difficult to convince such an enthusiast of error. As regards his astronomical speculations, and in particular the knowledge which he fancied he had received in visions concerning some of the heavenly bodies, we cannot do better than remind the reader of the point so ably brought forward by the authors of the "Unseen Universe." These gentlemen call attention to the significant fact that whilst Swedenborg gave the world an account of the planets whose existence was then known to the educated world, he is perfectly silent concerning Uranus and Neptune, which have been discovered since his time. Had he pointed out their existence he would have fully legitimised his claims to extraordinary knowledge, howsoever obtained. His having failed to do so must be considered as fatal.

As a specimen of Swedenborg's scientific writings we will quote, from his "Economy of the Animal Kingdom," a passage which Mr. Gorman characterises as "magnificent," and which he really seems to imagine is an answer to and an anticipatory refutation of Dr. Huxley's well-known work "Man's Place in Nature":—"It is especially to be remarked that all the wills and actions of animals—we mean all the instincts—are excited simply by external motives or moving causes, by those things that strike the senses, or that affect their blood in a general manner. The changes and conditions of the air and æther recurring with the four seasons send heat into their fluids, which burn and boil accordingly (!), and with the fluids as determinants a corresponding change is wrought in the organic forms of the body and brain. In this way the principle of motion is at once excited, and animals are carried agreeably to Nature's order into rational-seeming effects involving ends. Hence their loves, and hence the periods those loves obey. Hence the wonders they display in building their nests, incubating their eggs, and hatching their young. Hence their amazing parental care. Hence their public consultations as to the manner of providing for themselves and their progeny in the coming winter; and a number of other effects which proceed from a soul like theirs,

accommodated to the reception of life according to its own peculiar character, whenever it is excited by appropriate circumstances. Experience attests the truth of these remarks. For we know that the same effects are produced on animals by the warmth or heat of a room as by the heat of the sun, and when the season is neither spring nor summer. We may therefore say that the soul of animals resides in their blood, because it is always actuated by a cause extrinsic to itself. Not so the soul of man. He indeed is likewise moved, yet he is not governed by external causes. The affections of the external world pass *a posteriori* in some measure into the sphere of intelligence, yet in the man himself they are determined into act by a foregone will arising from an appropriate principle and cause. Thus we men are stirred to action by a fire kindled in the very sphere of the (intellectual) mind, even in mid-winter. As the philosopher (Aristotle) says:—'Whatever a secondary cause can do, a prior cause can also do in a higher and more noble manner. The first cause assists the second in its operations, and secondary causes are illuminated by the light of the first.' O! then, how obscured—how deeply buried in the grave of the body are the minds of those who judge of themselves by the brutes, and of their own souls by the souls of brutes, reasoning from likeness of actions, likeness of senses, and likeness of brain so far as the eye alone discloses the brain; and do not see beyond the likeness, nor how far we stand apart from them; fit subjects, indeed, for ridicule, did they not rather deserve our pity."

Now this magnificent passage may perhaps be rhetoric, though plain-spoken persons would probably consider it mere "padding." But it is assuredly not Science. It is what no earnest worker in Science could ever have written. The more closely we study it the less we feel disposed to expect anything of value from its author. In further exemplification of the same view we may refer to another passage on "the apparent resemblance and absolute difference between the brain of man and that of brute animals," and to a dissertation on the "essential distinction between man and brutes." Swedenborg—like his disciple and interpreter Mr. Gorman, and indeed like most men who study Nature in books, or in their own imaginings, instead of in things—was a profound believer in the absolute distinction and immense interval between man and "brutes." Our author accordingly does his poor best to be witty at the expense of Dr. Huxley. Says he—"The self-confident advocates of the 'brute' view of man's nature and origin cannot reasonably be offended if they are taken more or less at their word in this matter, albeit in a sense widely different from that intended by some who indulge their humour for writing 'Lay Sermons' for the enlightenment and edification of British working-men." The advocates of the doctrine of Evolution have more important duties in hand than feeling offended at Mr. Gorman's "takes" or mistakes. But we

may remind him that it is not the best and noblest man who is most ashamed of his poor relations.

We are here reminded of a further piece of evidence which proves that the work before us is in its spirit theological rather than philosophical. We refer to the manner in which Mr. Gorman treats all dissidents. He is not one of those who can agree to differ. He cannot apparently believe it possible for conscientious and enlightened minds to arrive at conclusions other than his own. All such he calls, in fact, with but little circumlocution, either knaves or fools, and imputes to them the most unworthy motives. No inconsiderable portion of the book is made up of such elegancies as the following:—"Meanest and most malignant subterfuges," "bestial fabrication," "wicked forgery," "offensive and slanderous epithets," "intolerable social nuisance," "flagitious example," "audacious and mischievous attempt," "wanton and profane hostility," "narrow-minded and arrogant class of intellectual obstructives," "the dark, narrow, tortuous, intolerant, and earthy tone and temper of Dr. Priestley's lucubrations," "the pseudo-scientific sect of Darwinian evolutionists," &c. Do not let the author, however, flatter himself that he can succeed even in offending the eminent men whom he denounces. They will give him that contemptuous pardon which is the privilege of a certain class. Says a German poet—

> "What cares the moon
> If a dog barks at her?"

If he wishes to criticise men of Science we would advise him to spend some eight or ten years at genuine scientific work, as a needful preliminary. He would then see not a few things in a very different light.

Thermo-Dynamical Phenomena, or the Origin and Physical Doctrine of "Life," and the New Theory of "Fermentation." By H. A. Huntley. Madras: Foster and Co. London: Longmans, Green, and Co.

The title of this pamphlet scarcely gives a full and fair idea of the author's objects. We quote, therefore, his opening paragraph:—

"The much-agitated question of the origin of 'Life' from organic liquids—or the spontaneous generation of Infusorial Animalculæ, Monad, Bacterium, and Vibrio, &c.—has always engaged the attention of careful observers; but owing to the indefiniteness (and hence conflicting character) of the results advanced no true scientific conclusion has hitherto been drawn. Moreover,

a majority of scientific men having been engrossed and subsequently gratified with the impulsive deduction drawn by Virchow —' that all known facts are opposed to the said theory of Generatio Spontanea;' and the remainder being absorbed by the vagary of the 'Germ theory' extant, proclaiming '*omne vivum ex vivo*;' analogous to the settled conclusions anent the natural processes of development of the sub-kingdom Vertebrata, Mollusca, &c., which patient investigation elucidated with regard to the origin of these; the real *cause* of 'Generatio Spontanea' has therefore been absolutely evaded, and hitherto involved in obscurity."

From this passage—which we can scarcely say gives us a very favourable impression of the author's habits of thought—we learn that he upholds the doctrine of Abiogenesis, and that he denies or doubts the presence, in the atmosphere and elsewhere, of organic germs, which under suitable circumstances may be developed. He repeatedly puts before us the question—"What is the real cause of Generatio Spontanea?—what is the cause of the development of these Infusorial Animalculæ from organic matter?" But before inquiring into the cause, it would surely be wise to be certain of the existence of the effect. This we do not see that our author succeeds in doing. We find no experiments here recorded where vegetal or animal life has arisen in solutions placed under such circumstances that no germs or seeds could have been introduced from without. Instead of such he tells us:—

"This conclusion which has been arrived at, of *heat* being the *cause* of spontaneous generation, has been ascertained by the following series of experiments, conducted, as will be noticed, with the most careful observation:—

"I. Any organic substance, either vegetable or animal, or both together, without previous boiling, subjected to the influence of and for the absorption of a *legitimate* temperature of heat (a subject I shall subsequently demonstrate), resulted in the production of living Animalculæ.

"II. I subjected a vegetable solution in cold water (without boiling), in a vessel, to a legitimate temperature of heat: after the lapse of some hours microscopic investigation evinced signs of active vivacity; in other words, Infusorial Animalculæ were skating about numerously, in active vivacity."

Now, that the author actually obtained these results our readers will scarcely question. No precautions were taken to destroy pre-existing life by boiling, or to prevent its subsequent introduction from without. By way of demonstration that the organisms developed in the liquid were not occasioned by "germs," we find the following passage:—"To prove my assertion that the said *germs* are *visionary* I cannot do better than quote Dr. Dougall, who recently asked—'What must be the size of the *germs* which give rise to *organisms* of about 1,400,000th

of an inch, and *less*, in diameter? and, further, if the said germs bear the same proportion to their adults as that which obtains between the *germs* of plants or animals familiar to us, and those when fully developed, the difference in some cases may amount to no more than tens of thousands, but in other cases it must be hundreds of millions. Compare, for example, an acorn with an oak; a turnip-seed to the large succulent bulb (!) it produces; the human *germ*, measuring about the 250th part of an inch, to a man weighing 16 stone; and it will be obvious that the *germs* of organisms in putrefying solutions, *if such exist*, are minute beyond comprehension, and that the *highest powers* of the *microscope* must ever be immeasurably inadequate to detect their presence.' These sentiments alone are enough to refute the theory *omne vivum ex vivo*, as regards the origin of the Infusoria and the lowest Fungi, as mould, or mildew, &c." With all due deference to the author and to Dr. Dougall, we cannot help regarding the man who can deny the existence of germs, on the strength of such considerations, a psychological curiosity. To doubt or deny the existence of anything because to our faculties or to our instruments it seems minute is an outcome of that vicious old principle which seeks to make man the measure of all things. The fact so often urged by the opponents of Abiogenesis,—that sealed tins of milk, meat, soups, &c., are not when opened found swarming with animal and vegetable life,—the author seeks to explain by saying that they "had not arrived at that stage to absorb a legitimate heat and result in life." Such phraseology seems to us the very essence of "vagueness." "A boiling heat or a frozen atmosphere are temperatures illegitimate and unregistered." Why the boiling heat should be called "unregistered," or what is the precise meaning of that term, we do not profess to decipher. But letting that pass; if "legitimate heat" denotes the temperatures below the boiling, but above the freezing point, we should think that a tin of meat, after being sealed and boiled in Australia, must, during its passage to England, have had every opportunity of absorbing "legitimate heat" enough. Yet we never hear of the development of Bacteria, &c., in the tins, except there is some breach of continuity admitting the outward air.

We do not see that Mr. Huntley has succeeded in throwing any new light on the very important and difficult question with which he has attempted to grapple.

Light as a Motive Power: a Series of Meteorological Essays. By Lieut. R. H. Armit, R.N. Vol. I. London: J. D. Potter.

The author announces his intention of proving that "there is only one law, one life, and one death in Nature, and that her

primum mobile is the universally felt power of light." He will also "endeavour to reconcile scientific discoveries with the account given in the Bible of all natural phenomena." As might be expected, from this declaration, we find in the book not a few remarkable statements. Thus the history of electricity, we are told, "dates back to the creation of the world." Electricity he seems to view not as a mode of force, but as a "fluid" capable of being decomposed into two elements. In the first Essay, entitled "A Retrospective Glance at Meteorology," we are particularly struck with the following passage:—"Whatever direction future inquiry may take it will ever be found, by the student of meteorology, that the Old Testament Scriptures contain more valuable data to go upon, leading nearer to the actual origin and cause of meteorological phenomena, than any other book in existence, *but the allusions to the laws of Nature, their operations, and effects, as contained in the Bible, are so masked and wrapped up in peculiar phraseology that the meaning, though peeping out all the while, yet lies concealed, and remains so until the light of Science is thrown upon it*, when it bursts out and strikes us with remarkable force, causing us to pause and ponder, and then to ask ourselves whether it be not true that 'that which hath been, it is that which shall be?'" We beg to call the especial attention of the reader to the words we have italicised, and which are, substantially, a confession that those men who receive the Bible as a physical revelation see in it whatever they wish, and in fact "read between the lines." This is a truth fully demonstrated by the history of Science. When the geocentric theory of the universe was still in vogue, divines—Protestant* no less than Catholic—proclaimed it to be a scriptural truth. When the Copernican theory triumphed, it too was held to be shadowed forth in the Bible. Even the nebular theory of Kant and Laplace, so long denounced as atheistic, is now declared—by no lower authorities than the authors of the "Unseen Universe"—to be involved in the sacred volume.

Perusing further this "Retrospect," we read of the Ethiopian merchants, who visited Judæa of old, sitting in an evening "enjoying the cool breeze, and, whilst *smoking their chibouk*, narrating the dangers through which they had passed in collecting those very goods that they had come to Palestine to sell, and thus instruct the Israelites." We were certainly not aware that the use of tobacco was at all known to the ancient Hebrews. Another historical mis-statement is to be found in the same section:—"After conquering the Gauls, the Roman legions under Cæsar invaded *Anglia*, as England was then called." The name Anglia was not applied to any part of Britain until after the withdrawal of the Romans.

* For instance, Dean Wren, the father of the distinguished Sir Christopher Wren.

Leaving, however, such minor points, we turn to the author's fundamental proposition that our globe is "surrounded by a transparent, spherical, metallic shell, enclosing within its folds the rising vapours from the earth, and preventing them from flying off into space, in the same way that the boiler-plates of an ordinary boiler enclose the steam which works the whole machinery." We naturally ask for the evidence upon which so extraordinary a statement is based. In reply, we are told that the atmosphere contains metallic gases, due to some obscure process of evaporation, constantly going on, by which all mineral bodies are more or less affected throughout the whole earth and the metallic vapour thus evolved—finding its way into the atmosphere—is thereby absorbed : the heat which creates the constant mineral volatilisation that turns the upper currents of our atmosphere into a metallic gas is derived not only from the sun, but also from the state of combustion in which it is believed the centre of the earth exists. Heat applied to water produces steam, which is absorbed by the atmosphere, and we have only to enter any foundry to see metallic vapour being likewise so absorbed. Under such circumstances it is only natural to conclude that all the minerals volatilised from a combination of causes find their way into our atmosphere. If they do not, whence come the aërolites which Nature precipitates at our feet, and which all reach the earth in the shape of boulders of highly-magnetised pure metal." Now, we admit the presence of water in the atmosphere, because we have direct proof of its existence. If "all minerals" are volatilised in a similar manner, their presence should be capable of demonstration. It would surely be sufficient to draw some thousand cubic metres of air by means of an aspirator through different solutions, such as hydrochloric acid, hydrosulphate of ammonia, &c.,—taking the precaution to exclude dust by a plug of cotton-wool at the entrance end,—and then snbjecting the liquids to a careful examination. That at certain temperatures all metals must be volatilised we are not disposed to doubt; but experience shows that as soon as the temperature is reduced below a certain point, these metallic vapours are re-condensed. Even if this were not the case, why should these supposed gaseous metals collect in the upper regions of the atmosphere? In what state or combination would they have to exist so as to be perfectly transparent and diathermanous, and yet be insoluble in the atmosphere? The meteorolites consist chiefly of iron, cobalt, and nickel,—the first-mentioned being widely diffused, indeed, but requiring a very high temperature for its volatilisation; the other two likewise very fixed, and decidedly rare. Why do we not find aërolites of the more volatile metals, such as lead or zinc? As soon as Lieutenant Armit, or any one else, shall demonstrate the presence of metallic gases in the atmosphere, by accurate chemical analysis, we shall receive it as a most interesting and unexpected

fact; but, even then, we shall require much further evidence before admitting the existence of a "transparent metallic shell" enveloping the globe.

The following passage is likewise worthy of attention:—"We account for its [electricity] being a fluid, pervading everything, in perpetual motion, always trying to find its own level, but never doing so, by the fact that we compare it to the 'heart of minerals,' which, being constantly taken away from them, requires continually to be replaced; and when given off, and not replaced, causes the mineral to fall or crumble away into dust. Many have seen this in the case of iron when—lying in the form of anchors, guns, cables, &c.—it is exposed to atmospheric influences which produce rust. In time the whole ponderous mass is converted into oxide of iron, which, nevertheless, can be revivified when again subjected to heat. Whilst undergoing the process of re-melting it throws off its oxygen into the carbon, and re-imbibes that which it had originally lost and thrown out into the atmosphere." This "heart of minerals" is something with which we should like to be better acquainted. Can our author prove its existence, or show its properties and its composition? Some minerals, indeed, crumble to dust by the loss of water; but this is certainly not the case with iron, which, to the best of human knowledge, does not give off anything, but, on the contrary, receives something when it rusts away under the influence of the atmosphere. Electricity, according to the author, is not a mode of force, but a substantive entity which he describes as a fluid, gas, or body (call it what you like). The view that aërolites are formed within the earth's atmosphere Lieut. Armit thinks is "proved by their containing one-third of all the simple bodies known to exist in the earth!" This, we must beg to say, is no proof at all, unless it could be shown that these simple bodies were exclusively peculiar to the earth. That they have contained hitherto no matter foreign to our globe is also no argument in favour of their terrestrial origin. If the suns and planets have been formed, as we now consider, by the condensation of cosmic nebulæ, we must naturally expect that their elements will be alike.

One of the author's views—which if well founded is capable of direct experimental verification—is that the compass is affected, in a manner not as yet ascertained, by, or at least during, foggy weather. To this abnormal variation he attributes the recent loss of the *Schiller*, off the Scilly Islands. This is surely a matter which requires prompt and careful investigation.

We are far from pronouncing this work to be absolutely worthless. It contains much interesting and useful matter, but it requires a careful revision and the expurgation of certain crudities. What, for instance, must the reader think who reads that "India never lacks moisture," and is then told—on the very

same page—that certain "vapour has gone to water Indian plains, where famine lately raged *owing to previous drought*"?

We hope to see the next edition freed from all the blemishes which we have felt it our duty to point out.

The Immortality of the Universe, considered in relation to the Persistence of its Motive Powers. By J. A. Wilson. Melbourne: G. Robertson. Auckland (N. Z.): Upton and Co.

The author combats the hypothesis that the solar system, and indeed the whole universe, must come to an end, as far as its motions and activity are concerned by the dissipation of energy. The nature of his speculations may be perceived from the following summary which we quote:—

"The planetary motions are uniform, notwithstanding there are media in space impeding them. Hence the planets must be supplied with energy to enable them to perform their motions with regularity. To find this energy we endeavour to trace it to or from the sun, whose internal economy we do not, however, understand, and cannot interpret; notwithstanding we are able to discern objections to other interpretations. We have to consider the nature and action of radiance in regard to the molecules and atoms of substances. Referring to the solar power there is a fundamental objection to the 'law of the dissipation of energy;' as, if extended to the whole creation, the death of the universe would follow. Hence we submit a theory, namely, that—

"I. The sun receives back, in a latent form, the light and radiant heat expelled by him, and that gravity may be taken as the exponent of this return force.

"II. Or that the sun draws his supply from a common store, accessible to other suns, to which they each contribute. Here, also, gravity may indicate the power that collects and draws the nutritive element to the sun. We ought to take an extra-planetary view in our effort to conceive the connection of radiance with its source, nor should we permit the terrestrial phenomena of oxidation and combustion to distract our minds. The undulations of radiance possess motive power; they enter into substances, some of which—as steam and chlorophyll—become media capable of retaining and transporting them. Media that absorb the undulations in a potential, and emit them in an actual form, appear to have been provided in nature where their presence seems necessary; and nowhere in the universe does such necessity exhibit itself more strongly than where the vast majority of those undulations vibrate, where their power may be stored, and whence it may be transported and turned to account

at the centres of activity. Such a medium may possess enormous space forces,—gravity which is inherent in matter being one of its modes, and forming a link between force and matter.

"The action of our ethereal medium, and the stream of radiance, may confer upon the planets their orbital and rotatory motion.

"The diurnal oscillation of the barometer is claimed as evidence of the external action of space forces upon our planet.

"This theory cannot explain the origin of the universe or the manner of the endowment of its motive powers; but it holds that an immortal universe is necessary as a habitat for immortal beings, and as a possession for its immortal Creator."

The Appendix on the Climatology of New Zealand is worthy of a careful examination.

The Creation; the Earth's Formation on Dynamical Principles, in accordance with the Mosaic Record and the Latest Scientific Discoveries. By Archibald Tucker Ritchie. London: Daldy, Isbister, and Co.

The Bible has been put to strange uses. Placed in one scale of a balance, and weighed against a woman accused of witchcraft, it was sometimes made to decide the question of her guilt or innocence. Hung up from a key, or according to some *with* a key inserted between the leaves, it was expected to point out a thief amidst a circle of bystanders. Opened at random, in imitation of the *Sortes Virgilianæ*, it was supposed to afford an augury as to the success or failure of any undertaking. But it is very questionable whether any of these obsolete superstitions involves a more fundamental misconception of the rightful claims of the Scriptures than does the notion—still lingering in the British Islands, especially in the lowlands of Scotland and the north of Ireland—of viewing them as a physical revelation, and of endeavouring to extract from them geological, chemical, or biological truths. Whenever, therefore, we meet with a so-called scientific treatise like the one before us, we know that the author has taken a radically false stand-point, and that we must prepare for a duty equally odious and unprofitable.

Mr. Ritchie is firmly convinced in his own mind of certain very strange propositions. He holds that the earth did not always rotate diurnally round its axis; that the sun, though the centre of gravity of the solar system, afforded no light; that the earth was a perfect sphere, covered everywhere "with a dark, unruffled mass of turgid waters;" and, above all, that there was no atmosphere! Under these circumstances, nevertheless, the waters were "the abode of innumerable races of living apulmonic

creatures," of which there were several successive generations, whilst other organic beings were subsequently formed "during the six working days of the Mosaic week, each day consisting of twenty-four natural hours!" The author has unfortunately forgotten to add that in those dark, atmosphereless ages two and two made five, and two straight lines were capable of inclosing space. All this we are to accept "upon the authority of the immutable word of God, assisted by the discoveries of Science!" Readers are further told that "unless they are prepared to receive the announcements of Scripture"—as interpreted and applied, of course, by Mr. A. T. Ritchie—"with as implicit confidence as they would a thrice-demonstrated problem, it will avail them little to accompany" him. The author does not, as far as we have been able to trace, adduce any original observations or experiments in support of his views. A large part of his six hundred pages consists of quotations from scientific works, but these chiefly represent the state of knowledge of the earlier half of the nineteenth century. The "Cabinet Cyclopedia," the "Bridgewater Treatises," the "Library of Useful Knowledge," Dr. Fleming, Hugo Reid, and Nichol, seem his favourite authorities. There is, of course, in his work much that is true, and altogether beside the question. But to find truths peculiar and essential to the author's system is a difficult task. We quote, as most significant, the following passage from a note on p. 409:—"It is also worthy of remark how frequently throughout the Sacred Volume the finger seems to have been pointed to those discoveries long, long ago; and repeated at intervals, throughout the whole course of the Divine Revelation, with a clearness only equalled by man's wayward reluctance to appreciate them. In proof of this the following are a few from amongst the numerous passages which might be quoted to show that, under the figure of 'stretching out the heavens like a curtain,' the expansive principle, now termed the 'diffusion of gases,' is as clearly indicated as if volumes detailing the results of experimental philosophy had been written on the subject:—Ps. civ., 2; Isa. xl., 22; xliv., 24; xlv., 12; li., 13."

With this writer any serious discussion is impossible. That such a work can have been written, and have gone through several editions, is a painful proof of the backward state of higher education in our country. But we accept the author's motto—"Magna est veritas et prævalebit." We feel assured that before another century has elapsed this book and its theories will be remembered merely as a lamentable instance of wasted labour and misdirected ingenuity.

Gregory's British Metric Arithmetic, for the High School, the Board School, the Desk, and the Counter. By Isaac Gregory, F.R.G.S. London, Paris, and New York: Cassell, Petter, and Galpin. Manchester: John Heywood.

The author of this work undertakes to show the superiority of "British metric arithmetic (of sub-units and whole numbers) over French metric arithmetic of prime units and large decimals." He remarks, very justly, that sooner or later—and better sooner than later—the British people must adopt the standard of the metre, which has been accepted by the majority of foreign nations, and adopted by the outer commercial world, as the common basis of an international system of weights and measures. He has undertaken to solve the question how this great reform is to be effected, and proposes a modification of the French system, which seems singularly exempt from objectionable features. Thus, for professional use he would adopt the French weights, intercalating merely in the table 25 grms. = 1 oz. and 500 grms. = 1 lb. His table of commercial weights would be—

25 grms.	=	1 oz.
20 ozs.		1 lb.
2 lbs.		1 kilo.
10 lbs.		1 stone.
10 stones		1 cwt.
20 cwts.		1 ton.

The kilo. of England being fixed the same as that of France, it is obvious that the new pound would be about $1\frac{1}{2}$ ozs. heavier than the present standard, whilst the ounce would be a little lighter. The French system, as in actual use, is inconvenient in all those small transactions which make up the sum of retail traffic. There is no unit available for mental calculation between 1 grm. and 500 grms. (livre or demi-kilogrm.), or, again, between 1 grm. and 1000 grms. (kilogrm.). In the same manner, in French money there is no available unit between 1 centime and 100 centimes (1 franc), though English residents get over the difficulty by calling the 10 centime piece a penny. In proof that "giving change rapidly, say at a crowded railway-station, is a difficult matter with French money," he asks—"Where do you find in any country, except France or other centesimal moneyed countries, a humble though useful officer standing at a railway-ticket office, who is looked upon as an almost preternatural ready-reckoner, whose public function is to tell the intelligent clerk inside the window, and the passenger outside, how much change to give and receive out of a 5-franc piece for two tickets of 1 franc 85 centimes each. Twice 185 is beyond the reach of the multiplication table." Decimals, the author declares, are charmingly easy on paper, but not well adapted for

mental calculation. He points out that "the mind, after finding the product by mental multiplication, which finishes the work in a system of integers, has to go in quest of the decimal points of the multipliers, reckon their conjoint value, and bring the conjoint decimal point down to the product, fix it correctly there between some two figures, and pronounce the money value of the product after this mental balancing." To do all this rapidly and correctly "in the head" requires a grasp of figures of which few minds are capable.

The French system is not, strictly speaking, "decimal:" in money it is centesimal, and in weights and measures millesimal. This serious difficulty Mr. Gregory proposes to avoid by the introduction of a metric ounce and pound, pint and gallon, and thus overcome the serious objection to the use of a decimal system.

We strongly recommend this work to our readers, who will find it much more interesting than the title might perhaps lead them to expect.

The High Antiquity of Iron and Steel. By St. John V. Day, Ass. Inst. C.E., F.R.S.E. London: W. H. Guest.

The subject of this pamphlet and the results at which the author arrives may be understood from the following paragraph:—"There is no escape from the conclusion that in all the earliest peopled countries—whether peopled by Semitic, Hamitic, Aryan, or Allophyllian races—there is most certain proof that in the remotest ages which we can ascertain anything about the inhabitants were familiar with the use and manufacture of iron and steel; that in those countries there is not a tissue of evidence in favour of a bone or stone age, still less of a bronze and then of an iron age succeeding; that from the evidence adduced, and which indeed is being continually supplemented, it is evident the stone, bronze, and iron theory must be consigned to the limbo of false ideas and exploded notions." Do the author's researches do anything more than push back the period of stone implements, which, after all, are realities?

List of Elevations, principally in that portion of the United States West of the Mississippi River. By Henry Gannett. Washington: Government Printing Office.

This pamphlet gives the altitudes of a list of towns, mountains, passes, and table-lands in the western portion of the American Union, as also those of a few in foreign countries. We notice

the wonderful elevation of New Mexico, Colorado, and Wyoming, in which the lowest town is situate at the extraordinary height of 4000 feet above the sea-level, whilst the highest —Montgomery, in Colorado—exceeds 11,000. The only error we perceive is in the height of Vesuvius, which, probably by a typographical error, is given as 8479 feet.

An Essay concerning Important Physical Features exhibited in the Valley of the Minnesota River, and upon their Signification. By G. K. WARREN. Washington: Government Printing Office.

THE author argues that Lake Winnipeg was at one time vastly larger than at present. At that period it discharged its waters, not as at present by the Nelson into Hudson's Bay, but by the Minnesota into the Mississippi; the Canadian lakes flowing, also, through Lake Michigan and the Illinois River, in the same direction. In support of this view he argues that the Nelson River and also the St. Lawrence bear, in their numerous rapids and falls, every mark of a comparatively recent origin. The Valley of the Minnesota, above its junction with the Mississippi, is out of all proportion too large for the former stream in its present condition, and only becomes intelligible if we consider it as the main artery of a vast tract of country. The Valley of the Illinois likewise appears disproportionate to the size of that river in modern times. These changes he accounts for by the assumption of a general subsidence of the land in the northern and eastern parts of the North American continent. By this change a new vent was opened for the waters of Lake Winnipeg and of the Canadian lakes, thus forming respectively the Nelson and the St. Lawrence, whilst the Minnesota and the Illinois, deprived of their main feeders, shrank into streams of an inferior rank.

The work is illustrated by several plans, and by a map showing the restoration of the ancient basin of the Mississippi. In this map the river is represented as rising in about lat. 56° N. and long. 40° W., draining the regions west of Hudson's Bay, passing through Lake Winnipeg, which then covered in all probability an area far exceeding that of Lake Superior, receiving the overflow of the Canadian chain through the Illinois, and falling finally into an estuary which penetrated to the influx of the Missouri.

The evidence in support of the author's theory has been very carefully collected, and is presented with great clearness.

Annual Report of the United States Geological and Geographical Survey of the Territories, being a Report of Progress of the Exploration for the Year 1873. By F. V. Hayden, United States Geologist. Washington: Government Printing Office.

This volume is especially devoted to Colorado, a region of great interest to the geologist. Amongst its valuable features we instance a catalogue of the minerals of Colorado, with their localities. Amongst these a prominent place belongs to certain tellurium compounds. These rare minerals occur at Red Cloud and Cold Spring, on both sides of a porphyry dyke, 50 feet in thickness, intersecting the granite, and striking about north-east. The tellurides of this region seem to show a greater variety of composition than those of any other locality hitherto known. Native tellurium occurs both massive and in small hexagonal crystals, with perfect lateral cleavage, forming columnar masses in white quartz. In hardness it ranges from 2 to 2·5. Its specific gravity is 5·802. It is lamellar in structure; tin-white to light steel-grey in colour; lustre splendent; streak sub-metallic, light grey to grey. It is less pure than the tellurium of Transylvania, containing only 90·85 per cent of tellurium, along with selenium, iron, and bismuth, and traces of gold and silver. Another telluride, occurring in the Red Cloud and Cold Spring mines, has received the name of Henryite. It is found in imperfect crystals, with good cubical cleavage, in thin threads, or in irregular foliated masses. Its hardness ranges from 2 to 2·5. Its specific gravity is 8·5253; its lustre metallic, splendent; colour bright silver-grey to steel-grey, but after exposure to the atmosphere for a short time it becomes a pale bright yellow. The streak is metallic, grey to silvery; it is opaque, brittle, partly malleable and sectile. Its composition is—

Lead	53·19
Iron	5·05
Silver	0·31
Gold...	trace
Tellurium (by difference)	41·45
	100·00

which leads to the formula $3PbTe+FeTe$, a small portion of the lead being replaced by silver. In its physical characters this mineral approaches Altaite, which contains 60 per cent of lead, and no iron. Schirmerite, another new mineral, is found in thin threads and foliated masses. Its cleavage is perfect; hardness, 1 to 1·5; lustre metallic, splendent; streak dark grey to black; colour between light lead-grey and steel-blue; opaque, partly malleable and sectile; flexible and thin scales. It contains 18·82 per cent of gold, and 28·60 of silver. The residue com-

prises, besides tellurium, iron and a trace of lead. The formula is probably $3AgTe+(AuFe)Te$. The ratio of Au to Ag is 2 : 3, whilst in Petzite, the mineral nearest approaching to it, the proportion is 5 : 8. Petzite, moreover, contains on an average 25 per cent of gold and 40 of silver. The number of mineral species associated with these tellurides is strikingly smaller than is the case at Nagyag.

There are further special reports on palæontology, on zoology, and on geography and topography. The zoological department embraces Lieut. Carpenter's report on the collections made on the Survey in 1873; destruction of pine timber in the Rocky Mountains; report on the Alpine insect-fauna of Colorado; list of butterflies collected in Colorado; on the geographical distribution of moths in Colorado; report on the Diptera of Colorado; notice of the galls collected by Lieut. Carpenter; lists of the Coleoptera, Neuroptera, and Myriapoda collected by the same explorer; and a variety of other valuable papers. Among the Coleoptera figures, of course, *Doryphora decemlineata*, the redoubted Colorado potato-beetle, which a certain morning paper—prone to occasional and indiscreet dabblings in Science—describes as "a kind of cockchafer." It has never been found westward of the great water-shed, and appears to be travelling slowly, but steadily, eastwards. It does not occur at greater altitudes than 6000 feet.

The work is profusely illustrated with maps, plans, sections, and views of remarkable geological features.

Report of the United States Geological Survey of the Territories. By F. V. Hayden. Vol. VI. Washington: Government Printing Office.

It appears that whilst each State of the American Union is executing a geological survey within its own boundaries, the Federal Government is performing the same task in the so-called Territories—the regions not yet formally organised. These surveys are being conducted in the most thorough-going and elaborate manner, and their Reports will prove documents of high and enduring value to the geologists, palæontologists, zoologists, and botanists of the whole world.

The volume before us—merely a small instalment of the fruits of this gigantic undertaking—treats of the Cretaceous Flora of the Western Territories. It is illustrated with thirty plates, containing several hundred well-executed figures of leaves, stones, seeds, &c., of the fossil species described, and belonging to the Dakota group. Of this Flora the author remarks that, "without affinity with any preceding vegetable types, without relation to the Flora of the Lower Tertiary of our country, and

with scarcely any forms referable to species known from coeval formations of Europe, it presents in its whole a remarkable, and as yet unexplained, case of isolation." He considers it probable that the first vegetable types, or at least the dicotyledonous ones, have appeared at the same or at different times, not only at different places, but with different original characters, constituting here and there distinct groups without homogeneity or relation of forms. Considering what is known of the succession of these groups, it seems as if some of the original types had persisted more or less indefinitely in the series, being modified perhaps by casual circumstances; and as if other original forms or prototypes had appeared here and there, and multiplied the characters of the vegetable groups." For a discussion on the origin of species, he considers the materials as yet too scanty. The flora of the cretaceous epoch alone, he remarks, comprises "many hundreds of thousands of species, comprised under more than fourteen thousand genera." If so—unless very different proportions prevailed between the various groups of the organic world from those which now obtain—the extinct insect-species of that epoch must have amounted to millions! But of these only units have left their traces in the "great stone book," which is in perhaps no other department so strikingly and so unfortunately imperfect.

A Manual of Electro-Metallurgy, including the Applications of the Art to Manufacturing Processes. By James Napier, F.R.S.E., F.C.S., &c. Fifth Edition, revised and enlarged. London: C. Griffin and Co.

This work, in its present form, commences with a history of the galvano-plastic art, including an account of the earliest experiments of Spencer, Jacobi, and Jordan, with a list of works written on the subject, and a notice of the patents taken out for improvements in the process. From this the author passes to a description of galvanic batteries, electrotype processes, the method of bronzing, miscellaneous applications of the deposition of copper, the deposition of metals one upon another, electroplating and gilding, and the results of experiments on the deposition of other metals as coatings. The concluding portion of the work is devoted to theoretical considerations. The author, as he states in his preface to the fifth edition, upholds the unitary view of the electric force, and in electrolysis holds that the electricity is "conducted through the solution by the base or positive element in the electrolyte, which it does as if it were a solid chain of particles or wire." The statements made in the first edition concerning the practical operations of the art have not, we are told, required alteration.

A Dictionary of Musical Terms. Edited by JOHN STAINER, M.A., Mus. Doc., Magd. Coll., Oxford, and W. A. BARRETT, Mus. Bac., St. Mary Hall, Oxford. 456 pp. London: Novello Ewer, and Co.

THAT the subject of Music should be ably treated by the learned organist of our great metropolitan cathedral will not be a matter of surprise, but under the title of "A Dictionary" few will be prepared to find a volume which can be read with pleasure, in addition to being an extremely complete work of reference on musical matters. The Editors have done wisely in calling to their assistance authors capable of writing on special subjects; and the reader in search of precise information will be glad to meet with such articles as "Temperament," by R. H. M. Bosanquet, M.A.; "Licensing" and "Copyright," by J. Bulley, M.A., Barrister-at-Law; "Ear, Larynx, Laryngoscope, and Structure of the Hand," by F. Champneys, M.A., M.R.C.S., of St. Bartholomew's Hospital; "Acoustics," by A. E. Donkin, M.A., F.R.A.S.; in addition to many others on subjects especially musical.

Six pages are devoted to the article "Acoustics," and the subject is most ably treated in so small a space.

Under "Larynx" the structure of the vocal organs and their functions are most fully described,—not only in man, but comparative anatomy is entered into in considerable detail. The structure of the larynx is illustrated with no less than twenty-one woodcuts, showing the structure of the parts concerned in vocalisation and the various positions they take when in action. The researches of Garcia and others are given at great length, as also a very full account of the laryngoscope and the mode of making observations by its means. The structure and comparative anatomy of the ear are treated in an equally careful and elaborate manner.

Under the head of "Fingering," in addition to the strictly technical matter, chiefly of interest to the pianist, will be found an interesting account of the anatomy of the hand in especial relation to its use on the fingerboard of keyed instruments.

The work is one of great value to lovers of music; almost all terms, whether English or foreign, are to be found in it, and a vast amount of information which can only have been the result of most indefatigable research on the part of the various editors. It is printed in the usual handsome manner of the publications of Messrs. Novello.

A Short History of Natural Science. By ARABELLA B. BUCKLEY, London: John Murray.

To furnish, within the compass of five hundred pages, a history of natural science, and of the progress of discovery from the

time of the Greeks down to the present day, may well be considered a bold undertaking, even though the work is primarily intended for the use of schools and of young persons. The authoress seeks "to place before young and unscientific people those main discoveries of science which ought to be—but which are not—known by every educated person, and at the same time to impart a living interest to the whole by associating with each step in advance some history of the men who made it." She adds—"I have often felt very forcibly how many important facts and generalisations which are of great value in giving a true estimate of life and its conditions are totally unknown to the majority of otherwise well-educated persons." This is true, and "pity 't is 't is true."

Upon the whole we may fairly congratulate Miss Buckley upon the success of her labour, and must declare that she fulfils, and often more than fulfils, her promises. She is not content to give a mere enumeration of discoveries in their chronological order. A number of phenomena and of inductions—astronomical, physical, chemical, and biological—are explained in clear and simple language, and are illustrated with diagrams. Thus a very small amount of preliminary knowledge is required from the reader, and no one will find himself repelled by a parade of needless technicalities. Of course, keeping within such narrow compass, and addressing herself not to the learned but to learners, the authoress cannot be expected to enter into minute details, or to give an exhaustive and authoritative judgment on the merits and claims of rival discoverers; but she furnishes a broad and mainly correct outline, which the thoughtful reader will doubtless seek to fill up.

The arrangement of the work is very simple. The first part describes the state of Science among the Greeks; the second treats of the Science of the Middle Ages, from A.D. 700 to A.D. 1500; and the third, and of course largest section, carries the reader down to the present day; each of the two former parts, and in the modern division the account of each century, being preceded by a list of its chief men of seience, with the dates when they flourished. Every chapter concludes with a list of the principal authorities consulted. As must be expected in a work of this nature, there are passages on which a variety of opinions may prevail; but in dealing with all controverted questions the authoress shows great moderation, and keeps within the truth. This is especially shown in her manner of dealing with the martyrs of science, and with the Darwinian controversy. In expounding the great doctrine of organic evolution she shows that it has no more connection with atheism than has the old and opposite view of the independent origin of species. The following passage is worthy the careful attention of all, however eminent, who have allowed prejudices to blind their eyes to the truth:—

"It is extremely foolish to be prejudiced against it, as some

people are, by the idea that animals formed in this way can be less God's creation than if they were made in any other way. The whole history of Science teaches us that men, in all ages, have constantly taken false alarm when it has been shown that God's ways are not our ways, and that the universe is governed by far wider and more constant laws than we had imagined in our little minds. But in the same way as the planets are none the less held in God's hand because we now know that it is by the law of gravitation that he governs their movements, so every plant and animal must be equally His creation, in whatever way they have been developed."

Surely this passage, equally reverent and philosophical, is a noble rebuke to all the frantic denunciation of Evolutionism which has been poured not merely from the pulpit and the political press, but which has also, *proh pudor!* been vented from the professor's desk and expressed in learned journals.

To one oversight we beg to draw Miss Buckley's attention:—The Roman general who was besieging Syracuse in the days of Archimedes was not Mæcenas, but Marcellus.

We can strongly recommend this book, not merely to the young, but to many persons of more mature age. It will, we think, give them a sounder, worthier conception of the achievements, the promises, and the claims of Science.

Proceedings of the Literary and Philosophical Society of Liverpool, during the Sixty-fourth Session, 1874-5. No. XXIX. London: Longmans and Co. Liverpool: D. Marples and Co. (Limited).

The gale which was stirred up by the ever-memorable "Belfast Address" of Dr. Tyndall seems not yet to have subsided, and is blowing very briskly in the pages of the volume before us. Three at least of the papers herein-contained—to wit, an address "On the Materialism of Modern Science," by A. J. Mott, President; an essay on "Potency in Matter," by the Rev. H. H. Higgins; and Part III. of a somewhat *buffo* memoir on "Philosophy without Assumptions," by T. P. Kirkman, F.R.S.—appear to be aimed, more or less directly, at Dr. Tyndall, Prof. Huxley, Mr. Darwin, Mr. Herbert Spencer, Mr. Justice Grove, and those mysterious bugbears the "Materialists." It was once the rule that scientific societies, metropolitan or provincial, should eschew metaphysics and theology, for the discussion of which there is surely ample room elsewhere; but at Liverpool it seems this prudent reservation, which after all is only a case of the division of labour, is thrown aside.

The very opening sentence of the President's Address has a strange ring about it:—"The time is near at hand, if we may

judge our age by its tendencies, when the pursuit of Science will have to justify itself anew to the reason of mankind." To the reason of mankind it requires no justification. Whether it ever will succeed in justifying itself to their prejudices and their stupidity is very doubtful. That happiness does not increase "in the world in the ratio of our intellectual acquirements" may be very true—but wherefore? Because the increase of knowledge has been simultaneous with other changes with which it has no necessary connection. For it is not the increase of our intellectual acquirements, or our intenser and more general devotion to scientific research, which narrows the margin between each man and want, and which makes the career of the vast majority a mere frantic struggle for existence. If the lives of learned men are "melancholy," is it not to some extent due to the manner in which they are treated by their fellows? Health may be injured in the workshop and the counting-house as well as "in the laboratory;" eyesight may be "dimmed" by poring over account-books as well as by gazing at the glories of the heavens; "time which never returns" may be spent on the Stock-Exchange as decidedly as in the "severities of study." Over-work, the bane of our age, is in no wise peculiar to men of science, and is in their case accompanied with far less anxiety than among men of business. Nor is it safe to assume that Science may with impunity be neglected by a nation because some men devote themselves to study by an irresistible impulse. We must not forget that the country which gives the greatest encouragement to discoverers will infallibly take the lead, alike in the arts of war and of peace. Into the remainder of this address we cannot enter.

Mr. H. H. Higgins takes up, again, the same subject. We can fully agree with him in his protest against the doctrine, recently revived, that animals are automata. Such a view might be pardonable in Descartes and Leibnitz, if a man may be pardoned for dogmatising on subjects which he has not made his especial study; but it is intolerable in Prof. Huxley, except he is prepared to prove that he himself is an automaton. We cannot, however, blame him for insisting that every proposed theory should be allowed to stand or fall upon its own evidence. What court of justice, in deciding upon the guilt or innocence of an accused person, takes into account, as a guiding element, the effect which such decision may have upon the welfare of others?

Concerning the doctrine of Evolution the author asks—"How is it there should be such difficulties, and whether their existence is not a significant fact?" Difficulties do not, so far as I know, beset in any like degree theories relating to things without life. "Theories in Astronomy, Chemistry, Light, Heat, Magnetism, &c., work smoothly enough." The answer to these questions lies in the far greater complexity and difficulty of the organic

sciences, and in the comparatively short time that we have had on these subjects anything worth calling a theory. In the inorganic sciences we have had to modify our theories, and have, in some departments, arrived at a close approximation to the truth. As Prof. Huxley aptly pointed out, it was at one time supposed that the planets revolved round the sun in orbits perfectly circular. This view explained certain facts, but there remained discrepancies, difficulties. The theory did not work smoothly. The orbits were then declared to be elliptical, and the discrepancies faded away. Is it unreasonable to expect that our zoological and botanical theories may be in like manner gradually improved?

So far we have been listening to men who at least were disposed to treat their subject seriously. But Mr. T. P. Kirkman is a writer of a different order: his object, apparently, is to make modern physicists and physiologists ridiculous to outsiders, and to demand for "parsons, poets, metaphysicians, and moralists" the liberty of laying down the law on matters which they have never studied. He jests and quibbles, and parodies and travesties the names of his opponents, pats them at times on the head in the style of a condescending schoolmaster, and praises a work of his own—doubtless with the kind intention of saving "trade-reviewers" the trouble. To criticise an author who is on such very excellent terms with himself would be labour lost. So we leave Mr. Kirkman, his "will," and his "philosophy without assumptions," which cannot be considered an unassuming philosophy.

From these arid regions it is a positive relief to turn to a paper on "Sponges, their Anatomy, Physiology, and Classification," by Mr. Thomas Higgin, which shows that some, at least, of the members of the Society are willing and able to produce valuable scientific work.

Descriptive Catalogue of the Photographs of the United States Geological Survey. By W. H. Jackson. Photographer. Washington: Government Printing Office.

If there still exists a man who entertains doubts concerning the splendid services which photography can render to all branches of natural science, let him look at the specimens accompanying this catalogue, and henceforth hold his peace. "By no other means," as the author with perfect truth declares, "could the characteristics and wonderful peculiarities of the hitherto almost unknown western half of our continent be brought so vividly to the attention of the world." No amount of description would tell the geologist one-half of what he can learn concerning the rock-formations of the regions illustrated by a mere glance at

these wonders of art. Nor could any draughtsman, however skilful and conscientious, accomplish, in any reasonable amount of time, the work here done by the camera in an instant. We need only cite, as instances in point, the view of "Cathedral Spires, Garden of the Gods," or of the "Eroded Sandstones of Monument Park." Perhaps the most wonderful production is the "Mountain of the Holy Cross." Such a happy rendering of a mountain tempest we should scarcely have imagined as possible. Through dense storm-clouds and rain-torrents a broad gleam of sunlight streams upon the brow of the mountain, and upon the mists boiling up in the ravines below. The view of the Upper Twin Lake, Colorado, if of less geological interest, is not less wonderful as a work of art. The reflections of mountains and trees in the lake, the evening sky, and the smoke-wreaths ascending from the camp-fire of the Explorers, are truly wonderful.

Nor are geology and physical geography the only branches of science upon which photography is thus shedding a new light. The archæology of pre-historic races, the ethnography of tribes still existing, and not less the zoology and botany of sparingly explored regions, may all share in the benefit.

We cannot better express our high opinion of these illustrations than by wishing that every scientific expedition, in what part of the world soever, may include among its staff a photographer as skilful as Mr. W. H. Jackson.

PROGRESS IN SCIENCE.

Mineralogy.—Deposits of crude borate of soda have been found by Mr. Robottom, of Birmingham, at the bed of a dry lake in the Slate Range Mountains. We learn, from the "California Alta," that the appearance of the surrounding country clearly indicates that water once stood 60 feet deep here over a large area, the ancient beach being distinctly traceable. The most remarkable fact about this saline product is, that in its middle there is a tract 5 miles long and 2 wide of common salt, while on the outside there is a deposit of borate of soda 3 feet thick, and under this a lower stratum composed of sulphate of soda and tincal mixed together from 1 to 3 feet thick. The borate of soda is easily crystallised, and on exposure of the crystals to the sun after they are taken out of the vat a white powder is obtained, which is preferred by some of the potters to the refined borax of the English market. For cleansing purposes this borate of soda is far more economical, and possesses great advantages over common soda and washing-powders.

At a meeting held at the Scientific Club, on the 3rd of February, it was resolved to establish a Society to advance the study of Mineralogy and Petrology. The name of the new Society is the Mineralogical Society of Great Britain and Ireland. The following officers have been elected :—President—H. C. Sorby, F.R.S., &c. Vice-Presidents—The Rev. Prof. Haughton, F.R.S., &c.; Prof. M. Forster Heddle, M.D., &c. Council—D. T. Ansted, F.R.S., &c.; The Rev. T. G. Bonney, M.A., &c.; Prof. Church, M.A., &c.; W. Crookes, F.R.S., &c.; Fred. Drew, F.G.S., &c.; Major Duncan, M.A., D.C.L., &c.; Archibald Geikie, F.R.S., &c.; Capt. Marshall Hall, F.G.S., &c.; Prof. T. Rupert Jones, F.R.S., &c.; Prof. Jas. Nicol, F.R.S.E., &c.; Prof. F. W. Rudler, F.G.S., &c.; R. H. Scott, M.A., F.R.S., &c. Trustees—The Right Hon. Lord De Blaquiere, F.R.G.S.; Patrick Dudgeon, F.R.S.E. Treasurer—R. P. Greg, F.G.S., &c. General Secretary—J. H. Collins, F.G.S. Foreign Secretary—C. Le Neve Foster, B.A., D.Sc., &c. Auditors for 1876—M. Hawkins Johnson, F.G.S.; B. Kitto, F.G.S.

It is proposed by Prof. E. T. Cox to distinguish, under the name of *Indianite*, a pure white clay which forms an extensive deposit in Lawrence Co., Indiana. The clay greatly resembles kaolin, and is worked for use in the porcelain factories of Cincinnati.

Under the term *Achrematite* Prof. Mallet, of Virginia, has described a new molybdo-arsenate of lead, occurring at Guanaceré, in Mexico.

Siegburgite is the name which Dr. A. von Lasaulx has applied to a new fossil resin found in the sands overlying the lignites of Siegburg, on the Rhine. It appears that the resin is found in various stages of oxidation, and its composition is therefore not constant.

In compliment to the great Italian geologist, Prof. Gastaldi, the name of *Gastaldite* has been bestowed upon a new mineral, by Prof. Strüver, of Turin. It is a silicate of alumina, soda, and protoxide of iron, crystallising in the monoclinic system, and occurring in the deposits of copper ore at Champ de Praz and S. Marcello, in the Val d'Aosta.

The Cornish mineral described a few years ago by Prof. Maskelyne, under the name of *Andrewsite*, has been analysed by Dr. Flight, and from this analysis the follwoing formula may be deduced :—

$$2(2Fe_2P_2O_8+Fe_2H_2O_4)+CuH_2O_2.$$

Dr. C. Le Neve Foster has read before the Geological Society of Cornwall some notes "On the Place and Mode of Occurrence of the Mineral Andrewsite." It appears from this paper that the mineral in question occurs in a tin lode coursing through granite, at West Phœnix Mine, near Liskeard.

Associated with some of the Andrewsite is a mineral which Prof. Maskelyne has referred to Ullmann's *Chalkosiderite*. Some crystals from West Phœnix have been measured, though with difficulty, by Prof. Maskelyne, who refers them to the Anorthic System, and publishes in the "Journal of the Chemical Society" a projection of the faces and a description of the crystals.

Attention has been called by Mr. Stoddart to the occurrence of gold and silver in carboniferous limestone, at Walton, in the neighbourhood of Clifton, near Bristol.

Dr. Hayes has submitted to the American Academy of Sciences a paper in which he traces the wide distribution of compounds containing phosphorus and vanadium through a great number of sedimentary rocks. Herr Hilger has lately determined the presence of lithium in a great number of sedimentary rocks.

To the "Journal of the Chemical Society" Mr. J. A. Phillips contributes a paper descriptive of the well-known pseudomorphic crystals which were found some years ago at Wheal Coates, near St. Agnes, in Cornwall. These crystals are pseudomorphs after twins of orthoclase, and are found, on microscopic examination, to consist, in some cases, of micaceous plates, particles of quartz, and crystals of cassiterite; whilst in others the cassiterite predominates, and is associated with blue tourmaline or indicolite. Analyses of both varieties are published by Mr. Phillips.

Mr. R. Pumpelly has described some pseudomorphs of chlorite after garnet, which occur abundantly in a bed of chloritic schist, overlying magnetite, in the Huronian Series, at Spurr Mountain Iron-mine, Lake Superior.

Some interesting crystals of Chondrodite, from the Tilley Foster Iron-mine, Brewster, New York, have been described by Mr. E. S. Dana. The mode of occurrence of the chondrodite at this locality has been described by Prof. Dana, but his son now publishes a memoir specially devoted to the crystallographic description of the mineral. The great interest of chondrodite lies in its relation to the Vesuvian humite, the two minerals being identical in chemical composition and exhibiting closely related crystalline forms. Three distinct types of humite have been recognised by Scacchi and Vom Rath, and Mr. Dana is enabled to show that all three types are paralleled in chondrodite. It is interesting to mark the influence of the Vienna school of crystallography on Mr. Dana, his memoirs—whether in Tschermak's "Mittheilungen" or in his own Journal—being illustrated by Miller's symbols, and with stereographic projections of the poles of the observed faces.

A zeolitic mineral, occurring at Neepigon Bay, on the north shore of Lake Superior, was described some time ago by Prof. Foote, under the name of *Zonochlorite*. Mr. Hawes has since analysed this substance, and finds it to be merely an impure variety of prehnite. In its mode of occurrence zonochlorite resembles the well-known *chlorastrolite*, which is found in amygdaloidal trap and in derived pebbles on Isle Royale, Lake Superior. After careful microscopic examination of chlorastrolite, Mr. Hawes concludes that it is not a homogeneous body, and that a large proportion of the stone consists of impure chlorite.

An analysis of a gypsum from White Mountain, in Southern Utah, has been contributed to the "Chemical News" by Dr. Machattie. The gypsum contains a notable proportion of carbonate of lime. An analysis of Peruvian *caliche*, or soda-nitre, is published by the same chemist.

The second Appendix to Prof. Dana's valuable "System of Mineralogy," prepared by Mr. E. S. Dana, has just been published, and brings the work up to January 1875.

Geology.—At the Annual General Meeting of the Geological Society, held on the 18th of February, the Wollaston Medal was presented to Prof. Huxley, in recognition of his distinguished services to geological science. The balance of the proceeds of the Wollaston Donation Fund was handed to Mr. J. Gwyn Jeffreys, for transmission to Prof. Guiseppe Seguenza, F.C.G.S., of Messina,

for his investigations upon the Tertiary beds of Italy and Sicily. The Murchison Medal was handed to Prof. Ramsay, for transmission to Mr. A. R. C. Selwyn, F.R.S., F.G.S., in recognition of his services to Silurian geology; the balance of the Murchison Geological Fund being also handed to Prof. Ramsay, for transmission to Mr. James Croll, in recognition of his valuable past labours, and in the hope that his enquiries would be still further prosecuted. The first Lyell Medal and the entire proceeds of the Fund were presented to Prof. Morris. The President prefaced his Anniversary Address by some obituary notices of Fellows and Foreign Members deceased during the past year, including Sir Charles Lyell, Mr. Poulett Scrope, Sir Wm. Logan, M. G. P. Deshayes, Mr. W. J. Henwood, Mr. W. Sanders, Archdeacon Hony, Sir Edward Ryan, and others.

In the "Quarterly Journal of the Geological Society," published in February last, is a paper by Mr. Thomas Belt, in which he describes the drift of Devon and Cornwall, and correlates it with that of the South-east of England. He divides the beds into two series, namely, Upland and Lowland deposits, and discusses their relation and origin. He shows that the gravels and transported boulders require the presence of water up to the highest level at which they are found, or about 1200 feet above the sea. He considers that this water was that of a great lake, formed by the bed of the Atlantic Ocean being filled with ice down to about lat. 39° on the American side and at 49° on the European side. This ice blocked up the English Channel, and with it all the drainage of Northern Europe, which was pounded back, and formed a great lake over which icebergs floated. The breaking away of the icy barrier caused the sudden and tumultuous discharge of the great lake, and the outspread of the Lowland gravels over the lower grounds. The author cites the known facts respecting the glaciation of Iceland, the Shetland Isles, the Hebrides, the North of Scotland, and the West of Ireland, as evidence in favour of his theory that ice did flow down the bed of the Atlantic from the direction of Greenland. He also argues that palæolithic man, the mammoth, and other large mammals, lived before the glaciation of the country, and not afterwards. They occupied, he thinks, the banks of a large river and its tributaries that flowed to the south-west, through what are now the Straits of Dover, before the advance of the Great Atlantic glacier. The theory of the pre-glacial age of palæolithic man, and also that of the bed of the Atlantic, having been occupied by ice, were brought forward by the author, in his paper on "Niagara," published in this Journal, in April, 1875.

Mr. J. Clifton Ward has recently communicated to the Geological Society the results of his investigation of the points of theoretic importance connected with the Granitic, Granitoid, and Associated Metamorphic Rocks of the Lake District. The paper is divided into five parts, the first four relating to the origin of the Plutonic rocks of the district, and the degree of alteration to which the surrounding rocks have been subjected. In Part I. the evidence of the liquid-cavities in the quartz of the granitic and granitoid rocks was considered, the general conclusion being that the granites, syenitic granites, and quartz felsites were all consolidated at very considerable depths, under great pressure, this pressure being much greater than could be due to the thickness of overlying rocks, and therefore exerted mainly from below and laterally, and resulting in the work of upheaval and contortion of the overlying strata. The period at which the principal formation of these granitic and granitoid rocks took place was considered to be that of the Old Red; and the work of elevation, consequent on the great surplus of upward and lateral pressure. was accompanied by an enormous denudation of rocks at the surface during the greater part of Old Red times. In the subsequent divisions of this memoir the mode of origin of these various masses was discussed. In Part II. the granites of Eskdale and Shap were dealt with, and it was shown to be at least probable, from evidence gathered in the field, and by microscopic and chemical examination, that these granites had been formed by the extreme metamorphism of rocks of the volcanic series, while at the same time the *partially* intrusive character of the Shap granite was suggested by various considerations.

Part III. discussed the origin of the Skiddaw granite from points of view furnished by field, microscopic, and chemical investigation. The gradual transition from unaltered Skiddaw slate to mica-schist was proved under these three heads, while at the same time the abrupt passage from the mica-schist to the granite appeared to negative the idea of the next step, into granite, necessarily following in this case. In Part IV. the quartz felsite of St. John's and the syenitic granite of Buttermere and Ennerdale were examined as to their origin; and there was found to be much evidence in favour of their representing transition beds between the volcanic series and Skiddaw slates, metamorphosed *in situ*. The interesting rocks of Carrock Fell were then considered, and field, microscopic, and chemical evidence were all thought to lead to the inference that these masses of felsitic, dioritic (?), and hypersthenitic rocks were due to the metamorphism *in situ* of the beds forming the lower part of the volcanic series. In Part V. the author points out several considerations relating to metamorphism, to which the geological facts of this district seem to lead. It would, he says, be "unwise to suppose that every granitic mass has been the root and origin of some past series of volcanic phenomena; for it may represent (1) a mass so deeply formed that, notwithstanding all the elevation and contortion produced by the intense pressure, no point of sufficient weakness was found whereby relief might be obtained and a volcanic centre be established; (2) a mass which, though very deeply formed, was yet able to penetrate upwards for a certain distance along some area or line of weakness, though its final point of consolidation was still far below the surface, with which it for ever remained unconnected by any volcanic neck; or (3) it may actually represent the very foundation of a true volcanic neck." The author also treats of what he terms "selective metamorphism," and concludes his memoir by summarising the principal conditions under which rocks occur with regard to metamorphism:—(1.) That state in which igneous fusion is the most important or conspicuous element. (2) A state of aqueo-igneous fusion, occurring at a much greater depth than the last, and reaching only a dull red-heat as a maximum. (3.) A state in which the rocks are permeated by water at a considerably lower temperature than 400° C.

A useful, descriptive, Catalogue of Rocks, Minerals, and Fossils, illustrative of the Geology, Mineralogy, and Mining Resources of Victoria, has been prepared by Mr. R. Brough Smyth, F.G.S., F.L.S., Secretary for Mines and Chief Inspector of Mines for the Colony.

We have received No. 6 of the Second Series of the "Bulletin of the United States Geological and Geographical Survey of the Territories," by Mr. F. V. Hayden, U.S. Geologist in charge. The Bulletin of the Survey was commenced in 1874, for the purpose of giving to the world, more rapidly than through the Annual Reports, the vast amount of new material which was constantly accumulating under the auspices of the Survey. The success attending the publication was so great that it commenced the year 1875 as a regular serial, and six numbers have been issued of about five hundred closely-printed octavo pages, with twenty-six pages of maps, sections, and other illustrations, with table of contents and full index.

Microscopy.—A new form of single magnifier has recently been contrived by Mr. John Browning. It is thus described by the inventor:—"The platyscopic lens is a triple achromatic combination, in which the chromatic and spherical aberrations are corrected by a central lens of dense glass. This lens is nearly three times as thick as the crown-glass lenses. The interior curves are almost hemispheres. The final correction for spherical aberration is made by altering the thickness of the dense glass lens. The three lenses are united by a transparent cement which has a refractive index corresponding very nearly with that of glass. This prevents light being lost by reflection from the surface of the deep curves. The platyscopic lens is made of three degrees of power, magnifying respectively 15, 20, and 30 diameters." Microscopists who have occasionally employed a low-power achromatic objective as a hand magnifier

cannot fail to have been struck with its brilliant definition: indeed the only drawbacks to its use have been its awkward form of mount and its great cost. The platyscopic lens admits of the same freedom in the manner of holding which makes the Coddington so convenient, while its freedom from chromatic and spherical aberration renders its definition vastly superior. Its working distance is also much greater than that of any lens of equal magnifying power: this is very perceptible in the smallest of the series, which gives so much space that opaque objects can be viewed with the greatest facility—an important matter to naturalists in field observations. The spines on the pollen of the Malvaceæ are easily seen, and a very good view is obtained through a considerable depth of water. The performance of the lowest power is wonderful for so small an amplification, and, from its large field and long working distance, makes a good lens for dissection or other minute operations, especially when mounted on a suitable stand. Owing to the perfect corrections of the platyscopic lens, the internal diaphragm made in the Coddington by grinding out the equatorial portion is not needed: the result is a vast increase in illuminating power, enabling the glass to work well in very bad lights. It is the most perfect single magnifier yet produced.

The address of H. C. Sorby, F.R.S., President of the Royal Microscopical Society (Trans. Roy. Micr. Soc., Feb. 2, 1876) contains a vast amount of interesting matter. The limit of the power of the microscope is discussed at some length, and especial attention drawn to the difficulty of distinguishing true structure from interference fringes when the intervals between the real markings are of the same order of magnitude as half the length of the waves of light. This effect is altogether independent of the quality of lenses. It depends on the physical constitution of light itself, and would only be the more perfectly seen with more perfect object-glasses. Some very intricate calculations are entered into with regard to the size of the ultimate atoms of matter. This part of the subject is too complicated to admit of an abstract. A careful perusal of the paper will repay those interested in so important a subject.

The subject of Micro-photography has been brought before the Medical Microscopical Society by Mr. G. M. Giles. The principal novelty is the way in which the employment of the heliostat is dispensed with. This is effected by the use of a long focus condenser, giving an image of the sun of such dimensions that it remains in field a sufficient time to permit of the object being photographed. The lens employed is an achromatic, photographic single combination, of 3½ inches diameter, aud about 10 inches focal length. The microscope is inserted into a "bellows" camera, capable of being drawn out to a length of about 2 feet, and of a size to take plates 6 inches square. The mirror is about 4½ inches square, is mounted so that its movements may be directed by means of cords running over grooved pulley: this is needed on account of the great length of the apparatus. The remainder of the fittings are of a kind that would suggest themselves to any practical photographer.

Dr. M. H. Stiles, in an account of a method of staining and mounting wood sections, calls attention to a few points of manipulation which may prove of interest to microscopists. For softening dried specimens for cutting, a mixture of equal volumes of alcohol, glycerin, and water is recommended. For bleaching previously to staining, if required:—A solution of ¼ ounce of chloride of lime in a pint of water, shaking occasionally for an hour, and, after allowing the sediment to subside, decanting the clear solution. After pouring off the bleaching solution, wash the sections by soaking them for at least twelve hours in water, changing frequently, and finishing off with distilled or rain water. The elimination of the chlorine will be much facilitated by placing the sections, after removal from the bleaching liquid, in a solution of hyposulphate of soda (1 drachm to 4 ounces of water) for an hour, and then washing as directed. Dr. Stiles, after staining, bleaching, and washing the sections, washes them three or four times with spirit, and, after draining, soaks them in oil of

cajeput * for an hour: at the end of this time the oil is removed, the sections drained on blotting-paper and immersed in turpentine: they are then ready for mounting in balsam or damar. Oil of cajeput is preferred to oil of cloves as being more limpid, much cheaper, and not giving its own colour to the tissue: it is freely miscible with spirit and turpentine, in all proportions.

Electricity.—Dr. Kerr, of Glasgow, has discovered a new relation between electricity and light. He finds that when plate glass is intensely dielectrified, and traversed by polarised light in a direction perpendicular to the lines of force, it exerts a partially depolarising action upon the light, giving an effect which is much more than merely sensible in a common polariscope. There is a good regular effect when the plane of polarisation is at 45° to the lines of force; no regular effect when the plane of polarisation is parallel or perpendicular to the lines of force. Electric force and optical effect increase together. The optical effect of a constant electric action takes a certain time to reach its full intensity, which it does by continuous increase from zero; and it falls again slowly to zero after the electric force has vanished. The dielectrisation of plate glass is equivalent optically to a compression of the glass along the lines of electric force. Dielectrified glass acts upon transmitted light as a negative uniaxal with its axis parallel to the lines of force. Dielectrified quartz, like glass, acts upon transmitted light as if compressed along the lines of force, while dielectrified resin, unlike glass, acts as if extended along the lines of force. Carbon disulphide is birefringent when dielectrified, acting upon transmitted light as glass extended along the lines of force. The electrostatic force and the birefringent power increase together; they also vanish simultaneously, the optical effect disappearing abruptly and totally at the instant of electric discharge. Of the liquids examined there are six which have given definite and constant results, namely, carbon disulphide, benzol, paraffin and kerosene oils, oil of turpentine, and olive oil. These bodies are distinctly birefringent when dielectrified, acting upon transmitted light as uniaxal crystals with axes directed along the lines of force, the uniaxal being negative in the case of olive-oil, positive in the other five cases. Dielectrified olive-oil acts in the same way as dielectrified glass, or as glass compressed along the lines of force; the other five liquids dielectrified act as resin dielectrified, or as glass extended along the lines of force. Compared among themselves, with reference to strength of birefringent action, the liquids appear to be very unequal—carbon disulphide the strongest, paraffin and kerosene the weakest. Compared with glass they are much weaker insulators; but if allowance be made for this difference, for intensity as well as purity of effects, carbon disulphide is far superior to glass. In contrast with glass, all the liquids are characterised by the absence of coercive force, and by the rapidity of variation of birefringent action from point to point of the electric field. The birefringent power is sustained in liquids by the present action of electric force at each instant: it seems also to be determined at each point, simply by direction and intensity of force at the point. The author enunciates the three following assumptions:—(1.) The particles of dielectrified bodies tend to arrange themselves in files along the lines of force. (2.) Changes of molecular arrangement, consequent upon rise or fall of electric action, are effected slowly and with difficulty in solids, easily and at once in liquids. (3.) The lines of electric force, or the axes of molecular files, are lines of compression in one class of dielectrics (glass, &c.), and lines of extension in another class (carbon disulphide, &c.). The facts, when thus interpreted, afford a strong confirmation of Faraday's theory of electrostatic induction; and in whatever way interpreted they give promise of some new insight into that interesting subject, the molecular mechanism of electric action.

Technology.—In his "Report on the Development of the Chemical Arts during the last Ten Years," Dr. Hofmann says that decolorisation is effected

* Distilled from the leaves of *Melaleuca minor*, a plant growing in the Molucca Islands.

less rapidly by peroxide of hydrogen than by chlorine. Tessié du Motay and Maréchal mention it as one of the agents which they propose for bleaching tissues, which, after treatment with permanganate of potash, they recommend to be steeped in a solution of peroxide of hydrogen. But it had been much earlier applied as a bleaching-agent by Thénard himself for a particular purpose—namely, for restoring old oil-paintings and drawings. White-lead in old paintings, which has become blackened by the gradual action of sulphuretted hydrogen, is converted into sulphate of lead by dilute solutions of peroxide of hydrogen, and thus restored to its primitive colour. A fine drawing by Raffaelle, with superimposed white which had become spotted with black, was completely cleansed by a solution which contained at most five or six times its volume of available oxygen, and the paper did not suffer. A peculiar application of this bleaching-agent has been made public by A. v. Schrötter and others. During the last few years bottles labelled "Eau de Fontaine de Jouvence, golden," and containing about 140 c.c. of a colourless liquid, have been sold by perfumers in great cities: to them, it appears, is due that offensive blonde shade of hair which holds an intermediate place between ash-grey and bright yellow, and attracts the attention of the spectators and the curiosity of observers by its *piquante* unnaturalness. This secret nostrum is merely a solution of hydrogen peroxide made stable by copious dilution, and by addition of a small quantity of acid—apparently nitric acid. A bottle costing 20 francs yields the purchaser 2·5 to 4 grms. of this substance in solution, and effects its purpose completely, though slowly, within four to six days, thus strikingly illustrating the great efficacy of peroxide of hydrogen. This would not be the first body whose industrial application commenced with trifles and gradually reached an unimagined extension. Nitrate of silver served first the vanity of the world as a hair dye long before its applications in photography. Schrötter very rightly expresses the wish that peroxide of hydrogen might be generally accessible at a moderate price. That for medicinal purposes it is preferable to oxygen and ozone is manifest. Whilst ozone only bleaches ivory in the strongest sunshine of summer, there is no doubt but that peroxide of hydrogen would answer the same purpose even in the absence of light.

Mr. Thomas Routledge, of Sunderland, who in 1860 was the only paper-manufacturer using esparto, the supply of which is now decreasing, has called the attention of paper-manufacturers to the probable advantages that would be derived from the employment of bamboo as a cheap and useful paper-making material.

Up to 1840 mirrors were silvered exclusively by means of an amalgam of tin—a process most destructive to the workmen employed. An important step was effected by an English chemist, Drayton, who conceived the idea of coating mirrors with a thin layer of silver, obtained by reducing an ammoniacal solution of nitrate of silver by means of highly oxidisable essential oils. This process was subsequently modified by several chemists, but only became really practical when M. Petitjean substituted tartaric acid for the reducing agents formerly employed. The glass to be silvered is laid upon a horizontal cast-iron table heated to 40°. The surface is well cleaned, and solutions of silver and of tartaric acid, suitably diluted, are poured upon it. The liquid, in consequence of a well-known effect of capillarity, does not flow over the edges, forming a layer of some m.m. in thickness. In twenty minutes the silver begins to be deposited on the glass, and in an hour and a quarter the process is complete. The liquid is poured off, the glass washed with distilled water, dried, and covered with a varnish to preserve the silver from friction. The advantages are evident. Mercury with its sanitary evils is suppressed; there is a gain in point of cost, as 4 to 5 grms. of silver, costing about 1 franc, suffice for 1 square metre, which, under the old system, would require 700 grms. of tin and the same weight of mercury. A few hours suffice to finish a glass on the new system, whilst the old process required twelve days as a minimum. On the other hand, the glasses thus silvered have a more yellowish tint; portions of the pellicle of silver sometimes become detached, especially if exposed to the direct action of the sun, and despite the protecting varnish the silver is

sometimes blackened by sulphuretted hydrogen. M. Lenoir has happily succeeded in overcoming these defects by a process alike simple and free from objections on sanitary grounds. The glass, silvered as above, is washed, and then sprinkled with a dilute solution of the double cyanide of mercury and potassium. The silver displaces a part of the mercury and enters into solution, whilst the rest of the silver forms an amalgam whiter and much more adhesive to glass than pure silver. The transformation is instantaneous. The amount of mercury fixed does not exceed 5 to 6 per cent. The glass thus prepared is free from the yellowish tint of pure silver: it is also less attacked by sulphur vapours and the rays of the sun, in which last respect it is superior to mirrors silvered by the old process.

An examination of French red wines for genuineness of colour has been made by M. Eugen Dietrich. The red of genuine wines appears brownish in thin layers; if diluted with 50 parts of water its colour is very faint; whilst artificially coloured wines appear decidedly blue-red, even at such a degree of dilution. If diluted with 20 parts by weight of water the behaviour of genuine wines as compared with spurious was as follows:—*Acetate of Lead* 1 : 10.—In genuine wines colour disappears; liquid becomes dirty and turbid. On heating, small silver-grey flakes with a slight reddish tint appear. Spurious wines yield large curdy flakes of a deep violet-blue, which on heating becomes more decided. *Sulphate of Copper* 1 : 10.—In genuine wines the colour disappears almost entirely; without turbidity. Spurious wines turn violet-blue, with a slight turbidity. *Baryta Water* 1 : 10.—Genuine wines lose their colour almost entirely; faint turbidity. Spurious wines turn violet-blue, or greenish blue and turbid. Filter-paper steeped in the above tests, and dried, becomes colourless if moistened with genuine wine, whilst coloured sorts give a violet or blue spot.

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I. ON THE GEOLOGICAL AGE OF THE DEPOSITS CONTAINING FLINT IMPLEMENTS, AT HOXNE, IN SUSSEX, AND THE RELATION THAT PALÆOLITHIC MAN BORE TO THE GLACIAL PERIOD.

By Thomas Belt, F.G.S.

CONCERNING the glacial period, geologists hold the most varied opinions, both with regard to its origin and to the mode of action of the ice. Thus at the very threshold of the geological record we tread on uncertain ground, and every guide points to a different path. The relation that palæolithic man bore to the great ice age might seem to be of easier solution; but even this question is unsettled, and a subject of controversy and doubt. Prof. Prestwich is believed by many to have proved that palæolithic man was post-glacial. Messrs. Croll and Geikie urge that there were two or more glacial periods in post-tertiary times, and that he flourished in a mild interglacial period. I, on the contrary, have been gradually forced to conclude that, in the British Isles, all the remains in caves and valley gravels referred to palæolithic man are preglacial, in the sense that they are of earlier date than the glaciation of the districts in which they are found.

I propose to state briefly some of the general arguments that have influenced my opinion, and then to deal with the special question of the age of the deposits at Hoxne, which the advocates of the post-glacial theory put forward as being undoubtedly in their favour.

Let us first take into consideration the age of the beds

containing the remains of the mammoth, the woolly rhinoceros, and their companions, with which the palæolithic implements are so often found. Wherever, in Europe, the relation of these beds to the boulder clay can be clearly seen, they are of distinctly older age. Thus, in Russia, Sir Roderick Murchison has recorded the discovery of the bones of the mammoth and woolly rhinoceros, near Moscow, in reddish clay covered with erratic blocks, on the plains 13 miles distant from the river.* And if we follow the northern drift southwards from Moscow, as I have done, we find it gradually changes from clay with boulders to the clay without boulders that covers the southern plains. Around the sea of Azof, cliffs of this glacial clay, 100 feet high, can be followed continuously for miles, and its junction below with the older beds is sharply defined. It rests on a freshwater deposit containing shells of species of *Unio*, *Cyclas*, and *Paludina*, and at this horizon fragments of the tusks and bones of the mammoth are abundant, and are always undoubtedly older than the glacial clay. In a similar position the same remains have been found at Odessa and other places in the South of Russia.

Nor has the theory of the post-glacial age of the remains of the mammoth remained unchallenged by eminent geologists in England. Prof. Phillips† and Mr. Godwin Austen‡ long ago recorded their conviction that they belonged to an earlier period than the deposition of the boulder clay, and that when they occur in newer beds they have been derived from an older formation. The remains are so plentiful in the caves of the North of England that it is certain that the mammoth and rhinoceros were abundant. Yet nowhere in the glaciated parts of the country have the bones been found excepting where preserved from the action of the ice in caverns and fissures.

Thus, in tracing the limits of the northern ice on the eastern side of England, I have found that Durham and Northumberland were probably completely overflowed by it, excepting the upper parts of the Cheviots, as pointed out to me by Mr. Richard Howse. The ice streamed through from the west, around the southern and northern flanks of the Cheviots, down the valleys of the Tyne and the Tweed, and when approaching the eastern coast was deflected to the south by the great mass of ice that occupied and was flowing down the bed of the German Ocean. In Yorkshire the ice

* Geology of Russia in Europe, p. 650.
† Geology of Yorkshire, 1829, vol. i., pp. 18 and 52.
‡ Brit. Assoc. Reports, 1863, p. 68.

from the west was held back by the Pennine Chain, and did not coalesce with the German Ocean glacier, but stopped short, somewhere about an irregular line drawn from Keighley, north-eastward to near the mouth of the Tees. The German Ocean glacier only, as it were, grazed the high land bordering the coast until it reached the northern shores of Norfolk that stood out across its track. A large portion of Yorkshire was thus never glaciated by land ice, and in this area remains of the great extinct mammals have been found in and below the lowland gravels, as at Leeds and Market Weighton; but when we pass north-westward into the country where the striæ on the rock surfaces bear witness to the passage of land ice, no such remains are found, excepting in caverns and fissures of the old rocks.

The north-western side of England is much more glaciated than the north-eastern, and the mammalian remains have only been found where preserved in caves. The ice filling the Irish Sea reached to a height of 2000 feet on the western flank of the Pennine Chain. Probably reinforced from the westward it continued, in scarcely decreasing thickness, across the whole of Lancashire and Cheshire, and passed over into the drainage area of the Severn, down which valley it appears to have flowed for some distance. As soon as we get beyond its influence we again meet with mammalian remains in the lowland gravels, and in most of the southern valleys they are abundant.

If the mammoth and its associates roamed as far as the north of England, and even into Scotland, after the glacial period, their remains ought to be found in the valley gravels of the glaciated districts. They are, however, absent, and if we should be led to infer from this that they lived before the glaciation of the country, and accept the conclusion of Prof. Phillips and Mr. Godwin Austen that the mammoth and the woolly rhinoceros lived before and not after the glacial period in Great Britain, we can scarcely refrain from going farther than these geologists and concluding that the makers of the palæolithic implements were also preglacial. For no geological inference seems based upon sounder evidence than that palæolithic man was contemporaneous with the mammoth and its associates. The implements of the one and the bones of the others are found together in the same stratum of the cave earth, and in all the numerous caverns that have been searched in England and Wales, there is no record of palæolithic implements being found at a higher horizon; when flint weapons do so occur they are invariably of the neolithic type. If geological evidence of

contemporaneity is of any value, the occupation of the caves by palæolithic man ceased at the same time as the great mammals disappeared.

Let us look at the question from another point of view. In the south of England the remains of the mammoth are abundant in the valley gravels. They are found mixed through them or more commonly at their base. Palæolithic implements are found in the same position, though usually in gravel higher up on the slopes of the valleys. When found in the gravel, the bones are broken and worn, and the flint implements have their angles rounded more or less as if by rolling. When, as has happened in a few cases, the bones and implements have been found below the gravels, they have been uninjured and unworn. Mr. Godwin Austen noticed the occurrence of bones of the mammoth in an old forest bed beneath the valley gravels, at Peasemarsh, in Surrey, uninjured and lying together, whilst in the overlying gravel, the teeth of the mammoth were found singly and rolled.* And Colonel Lane Fox has recorded the discovery of flint implements at Acton in seams of white sand, 9 feet from the surface, beneath deposits of gravel and brick-earth.† Their edges were as sharp as if just flaked off a core of flint, whilst those found in the gravel, on the contrary, have their edges worn and rounded just like those of the subangular pebbles of which the gravel is principally composed.

The position and the state of preservation of the bones and implements are such as might be expected if they had been deposited on an old land surface before the outspread of the gravels, when the configuration of the country was much the same as now; and I have suggested that the occurrence of the implements, generally higher up the slopes of the valleys than the mammalian remains, is due to palæolithic man having frequented more elevated and drier localities than the great mammals. I have urged that the outspread of the gravels was due, as formerly supposed by Sedgewick, De la Beche, and Murchison, to the action of a great flood or debacle. I have advanced the theory that that debacle was caused by the breaking away of a barrier of ice that blocked up the English Channel, and with it all the drainage of Northern Europe, causing an immense lake of fresh or brackish water that was thus suddenly and tumultuously discharged.‡

* Quart. Journ. Geol. Soc., vol. vii., p. 288.

† *Ibid.*, vol. xxviii., p. 456.

‡ Quarterly Journal of Science, April, 1875. Quart. Journ. Geol. Soc., vol. xxxii., p. 84.

This great flood occurred, according to my theory, before the culmination of the glacial period, and was primarily due to ice filling the bed of the North Atlantic as far south on the European side as lat. 49°. If the gravels in and below which the rude flint implements and the remains of the extinct mammals are found, were thus spread out, it follows that they were preglacial in the sense that they lived before the principal glaciation of the country.

We have seen that in the north such an excellent geologist as the late Prof. Phillips had arrived at this conclusion with regard to the age of the mammoth, the woolly rhinoceros, and the hippopotamus, and in the south, Mr. Godwin Austen, from a study of the same remains in the valley gravels. Direct evidence of great value has been added by Mr. Tiddiman in his reports on the exploration of the Victoria Cave, at Settle. He has shown that the cave deposits lie beneath glacial clay and amongst the other remains a human fibula has been found.* In the Cefn Cave, in Denbighshire, Mr. Mackintosh has also determined that the mammalian remains lie in and below a glacial clay.†

All the lines of enquiry thus far pursued in this paper point to the pre-glacial age of the remains in question, and some of the facts are directly opposed to the post-glacial theory. How then is it that the great majority of geologists write as if it had been clearly proved that palæolithic man was of post-glacial age? Principally because it is believed that Prof. Prestwich has proved that at Hoxne, in Suffolk, the implements and bones are found in deposits distinctly overlying boulder clay. This is spoken of as if it were a truism in most general treatises on geology;‡ and both in Europe and America the presumption is appealed to as being conclusive with regard to the age of the remains. The general opinion held is concisely given in the statement by Mr. John Evans in his Presidential Address to the Geological Society last year, that at Hoxne "the implement-bearing beds repose in a trough cut out in the upper glacial boulder clay, which itself rests on middle glacial sands and gravels."||

This opinion of the age of the Hoxne deposits is founded on the elaborate memoir by Prof. Prestwich, published in the "Philosophical Transactions of the Royal Society," for

* Nature, vol. ix., p. 14. Brit. Assoc. Reports for 1873, 1874, 1875.

† Quart. Journ. Geol. Soc., vol. xxxii., p. 91.

‡ Sir Charles Lyell, Antiquity of Man, p. 166. J. Geikie, Great Ice Age, p. 474. J. Croll, Climate and Time, p. 241. W. Boyd Dawkins, Cave Hunting, p. 410. Jukes's Students' Manual of Geology, p. 736.

|| Quart. Journ. Geol. Soc., vol. xxxi., p. 74.

1860. In this treatise the author gives a diagram showing the deposits in question lying in a trough cut out in the boulder clay. Though this section is confessedly only theoretical, it was accepted by Sir Charles Lyell and others as an actual one, and afterwards the author himself wrote as if he had proved his theory to be true,* which he may well be excused for having done, when it had been accepted by so many eminent geologists.

The writings of Prof. Prestwich are admirable in this, as in other respects, that although he indulges in wide-reaching theories he invariably gives the evidence on which they are founded. Thus, in the memoir in question, in addition to the theoretical diagram he gives another, showing the actual facts observed, and also careful details of the various sections observed by him. It is therefore possible to check his theory by his facts, and in the present paper I shall do so, and also give the results of my own examination of the Hoxne district.

Mr. John Frere, so long ago as the first year of the present century, communicated to the Society of Antiquaries an "Account of Flint Weapons discovered at Hoxne, in Suffolk."† He stated that they were found in great numbers in a bed of gravel which was overlaid by 1 foot of sand with shells, and containing the jaw-bone and teeth of an enormous animal; the sand being again covered by $7\frac{1}{2}$ feet of brick clay. Mr. Frere noticed that the strata lay horizontally, and had been denuded to form the present valley, and therefore concluded that they belonged to a period when the configuration of the surface was different from what it is now, and he considered that their antiquity was possibly "even beyond that of the present world." The manner in which the flint implements lay, and their great abundance, led Mr. Frere to conclude that a manufactory of them had been carried on at the place where he found them.

The discovery does not appear to have excited any attention at the time, and for more than half a century remained unnoticed. In 1859, when the discovery of flint implements in the Valley of the Somme, in France, in association with the remains of the mammoth and other extinct mammals, had at last aroused the attention of geologists, Mr. Frere's memoir was brought by Mr. John Evans before the notice of Mr. Prestwich, who had just returned from Amiens. He soon after visited Hoxne, and carefully examined into the facts of the case. He found that the bed of brick clay was

* Philosophical Transactions, 1864, p. 253.

† Archæologia, 1800, vol. xiii., p. 206.

still being worked, and that flint implements were occasionally, though rarely, turned up; and on a subsequent visit with Mr. Evans they succeeded in disinterring one themselves.

The valleys of the Waveney and its tributaries are bounded by low hills of gravel and boulder clay. The bed rock is not seen in any of the sections exposed, but it is supposed to be chalk. The gravels and sands (the middle glacial sands and gravels of Mr. Searles Wood, jun.) are exposed in many gravel-pits on both sides of the Waveney. They are sometimes capped by the upper boulder clay; at others, by a more sandy bed with stones (the "trail" of Mr. Fisher), which in some of the sections graduates into the upper boulder clay, of which I believe it to be the modified representative. One of the deepest sections on the north bank of the Waveney is near the road from Diss to Harleston, at Billingford, where the series of beds shown in Fig. 1 are exposed.

Fig 1

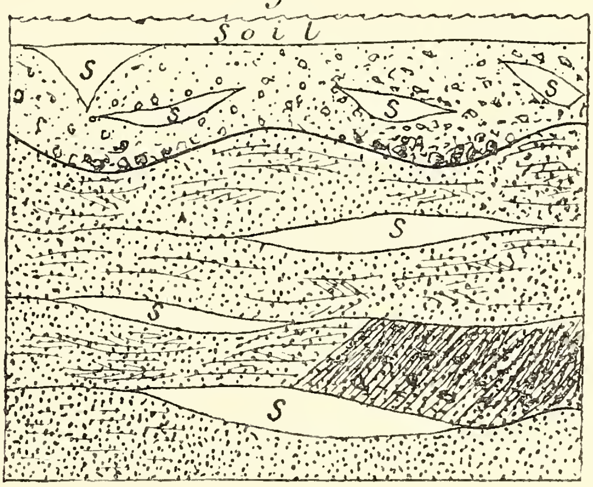


Fig. 1.—Scale 12 feet to 1 inch. 1. Sandy clay or "trail," with patches of sand (s) and scattered flints, mostly in nests, at the irregular base of the deposit. 3. Sands and gravel, false bedded with lenticular beds of sand (s), and in the lowest seams rounded pebbles of chalk.

Mr. Fisher some time ago called attention to the great importance of the upper bed, or "trail," in the study of the glacial beds,* but it has not yet received the notice it deserves. It is the most persistent of all the beds in the South-eastern counties, and can be traced, in almost every section, from Norfolk into Surrey. It is everywhere seen in the Thames valley lying on the top of the lowland gravels, and is shown in great perfection in the long section now (March, 1876) exposed between Acton and Hanwell, on the

* Quart. Journ. Geol. Soc., vol. xxii., p. 553.

Great Western Railway. It generally, if not always, rests upon an irregular surface of the beds below it, and contains stones derived from some other source.

On the south side of the Waveney, at Syleham, there are good sections on both sides of the turnpike, and these exhibit similar false-bedded sands and gravels, which are, however, covered by the upper boulder clay instead of by "trail." Fig. 2 shows a section exposed on the south side of the turnpike. A little further west, on the north side of the turnpike, is another gravel-pit, showing a similar succession, but with the beds of sand and gravel strongly false-bedded. In all these sections small pebbles of chalk are very abundant in the lowest beds. The most remarkable feature in the Upper Boulder Clay is the numerous angular patches of material quite different from the matrix of brown clay. The angular patches of red sand are very peculiar, and difficult to explain.

Fig. 2.

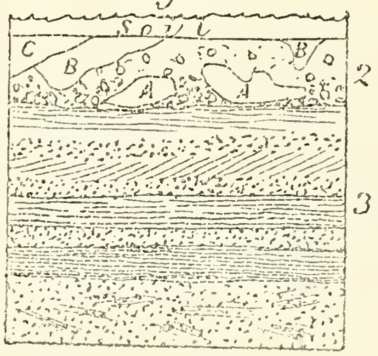


Fig. 2.—Brown boulder clay, with many whole flints, and with angular patches of red sand (B), marly clay with small stones (A), and red boulder clay (C). 3. Sands and subangular flint, gravel with rounded pebble of quartz, and (in the lowest seams) of chalk.

In a large gravel-pit a little north of Oakley Church there is a long section exposed, and in it the Upper Boulder Clay, similar to that shown in Fig. 2, at one end of the pit, gradually changes into a sandy loam with stones and angular patches of sand, not to be distinguished from the deposit named "trail" in Fig. 1.

At Hoxne itself, on the east side of Gold Brook, there is a gravel pit showing seams of gravel and sand exactly similar to that at Syleham, but surmounted by sandy "trail" instead of by boulder clay. The gravel is not to be distinguished from the other, being composed like it of sub-angular flint pebbles with rounded ones of quartz and quartzite, and with many small pebbles of chalk in the lowest seams. Notwithstanding this great similarity, Mr. Prestwich considers the beds at Hoxne to have been formed by river action

in post-glacial times, whilst those at Syleham, being capped by boulder clay, he of necessity classifies as middle glacial. Yet I could find no difference whatever in their appearance or composition. In both, the pebbles are mostly small and subangular, with some rounded ones of quartz and quartzite. Both contain many small pebbles of chalk in their lowest seams, and both are false-bedded. That one is covered with boulder clay and the other by sandy " trail " does not suffice to prove them of different age, for at the Oakley gravel-pit we can trace the same gravels from one end, where the boulder clay overlies them, to the other, where the " trail " does so. The middle sands and gravels are generally supposed by geologists to be marine, and it is incredible that deposits due to such different agencies as that of the waves of the ocean beating on a beach, and that of a flooded river, should be absolutely identical in appearance and composition. But nowhere is either the ocean or any river known to be forming deposits of subangular pebbles, excepting where they are cutting into pre-existing beds of the middle glacial series. Both in sea and in river beaches the pebbles are smoothly rounded, and not, as in the gravels under consideration, broken and subangular. Even when we find in the latter rounded pebbles of tertiary age, there is often a piece chipped out of them, as if they had been dashed violently together. I have had a large number of the pebbles from the gravel at Ealing counted, and find that over 80 per cent are broken or subangular. I ask where, in the whole world, is such a deposit being formed by existing agencies? Surely if ordinary floods would produce them they have had plenty of opportunities of doing so during the past pluvial year, yet where, on the banks of any of our rivers, have the great floods left deposits even approaching in character to those that geologists confidently ascribe to river action? That they were caused by a great flood I fully believe, though not to that of any river, but to one that swept over the whole country, driving a huge mass of gravel and sand, and leaving them mantling both hills and valleys, holding or covering up the remains of palæolithic man and the great mammals that had lived before the waters were pent up by the Atlantic glacier.

A little above Hoxne, on the left side of the stream called the Gold Brook, is the Hoxne clay-pit. The clay is excavated along the slope of the shallow valley through which the brook runs. The road to Eye skirts the hill side, having to the west the park of Sir Edward Kerrison, and to the east, between it and the stream, a narrow strip of land from which

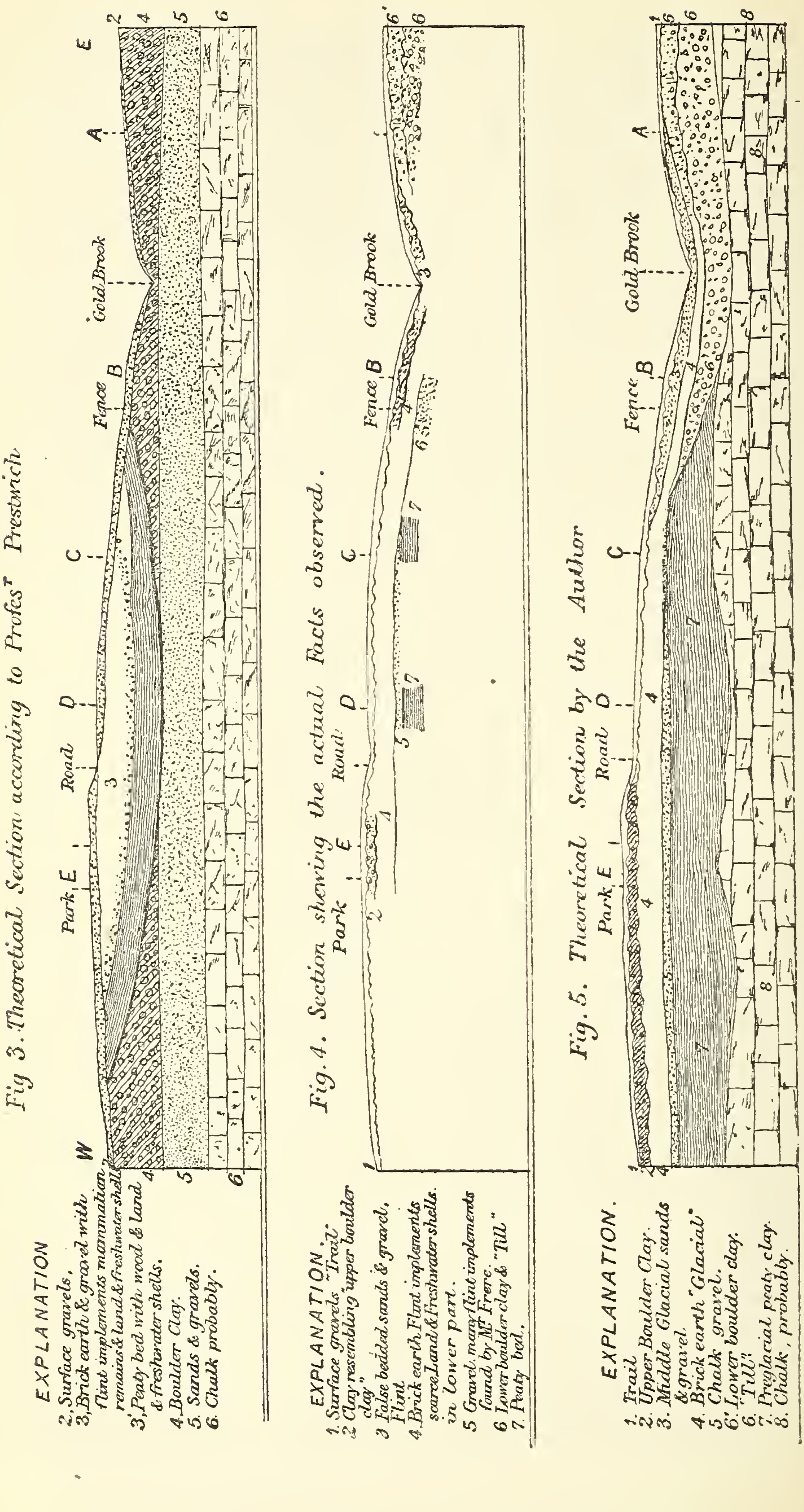


Fig 3. Theoretical Section according to Profes^r Prestwich

Fig. 4. Section shewing the actual Facts observed.

Fig. 5. Theoretical Section by the Author

the clay has been dug. The old workers had commenced near the village of Hoxne, and as they gradually exhausted the clay up to the road they moved further southward, and the point at which it is now excavated is probably at least a quarter of a mile distant from that where Mr. Frere made his discoveries in 1800. The pit has now been worked up to some farm buildings that interfere with its progress southward, and to get clay they have now crossed the road into the park, and thus made a most important addition to the section laid open.

I have in the accompanying plate given three sections of the ground. The first shows the theoretical relation of the beds according to Prof. Prestwich. The second exhibits the facts actually observed by Prof. Prestwich and myself; and the third is a theoretical section, showing the relation that the beds hold to each other according to my own views. We shall in the first place confine our attention to the second section, Fig. 4, showing the facts actually observed.

Fig. 6

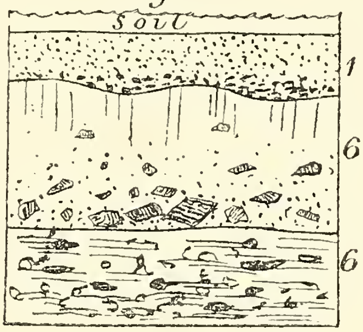


Fig. 6.—1. "Trail," 3 feet. 6' and 6. Boulder clay, chalky in upper part. A slight line of division between it and the lower part, which is principally composed of crushed Kimmeridge clay with pieces of chalk.

On the east side of Gold Brook a cutting has been made into the bank, and a thick bed of boulder clay is exposed. At the point A in general section the beds are shown as in Fig. 6. Near the line of division the upper and more chalky clay contains many large flints and transported boulders. Some of these are smoothed, and strongly scratched and grooved. Two scratched blocks of septaria that I saw measured 1½ feet across. This boulder clay, both in its upper and lower division, is very distinct in appearance and composition from that lying above the gravels as seen in other sections. Lower down towards the brook a seam of false-bedded sandy gravel comes in between the boulder clay and the "trail," and represents, I think, the gravels of Figs. 1 and 2.

Crossing the brook and ascending the opposite slope, we

have, at the points C and D of general section, typical sections of the clay-pit, as shown in Fig. 7. The clay, 4 in section, is called "red brick earth" by the workmen, because it burns to a red colour; whilst the lower dark-coloured clay, 7 in section, is called "white brick earth," because it burns to a white colour. The bottom of the latter bed has not been reached, although Prof. Prestwich had a boring put down into it to a depth of 17 feet. It is full of vegetable matter, and I found numerous pieces of wood in it. The men pointed out to me the gravel seams, 5 in section, as the horizon at which flint implements had been found; but shortly before Prof. Prestwich visited the pit, two specimens had been taken from the lower part of the clay, 4 in section. There can be little doubt, however, that they were found by Mr. Frere in the gravel below the "red brick earth," as he says that—"They lay in great numbers at the depth of

Fig 7

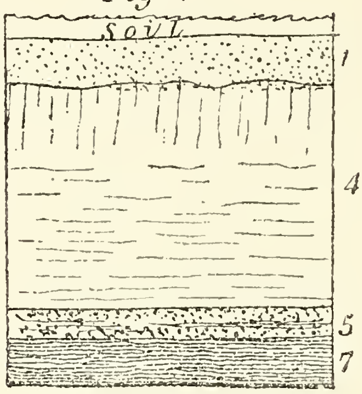


Fig. 7.—1. Sandy "trail" with flint pebbles. 4. Yellowish brown clay, unstratified at top and graduating downwards into obscurely stratified chalky clay: 10 feet. 5. Two thin bands of small chalky gravel, separated by 8 inches of loam. 7. Dark calcareous clay, with fragments of wood and other vegetation.

about 12 feet in a stratified soil, which was dug into for the purpose of raising clay for bricks. Under a foot and a half of vegetable earth was clay seven and a half feet thick, and beneath this one foot of sand with shells, and under this two feet of gravel, in which the shaped flints were found generally at the rate of five or six in a square yard. The manner in which the flint implements lay would lead to the persuasion that it was a place of their manufacture, and not of their accidental deposit. Their numbers were so great that the man who carried on the brick-work told me that, before he was aware of their being objects of curiosity, he had emptied baskets full of them into the ruts of the adjoining road."

As I have already mentioned, the place at which the clay is now excavated is some distance from that where Mr. Frere

found the implements, and they are now very seldom met with,—so seldom that none of the men working at the clay-pit when I was there had ever seen one.

To the west of the road, in the pit that has been opened in Sir Edward Kerrison's park, a section of the beds has been exposed at the point marked E in general section, as shown in Fig. 8. The most remarkable feature in the section is the occurrence of the upper clay, 2 in section, containing angular patches of red sand, like that seen in the "upper boulder clay" of other parts of the district. I cannot help thinking that if this section had been open when Prof. Prestwich examined the deposits he would have been

Fig. 8.

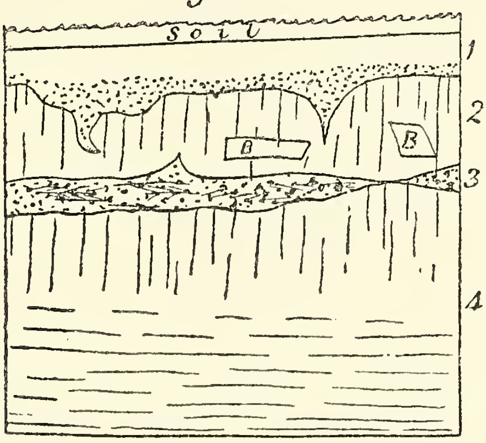


Fig. 8.—1. Sandy "trail" with flints graduating downwards into sand, filling pipes in clay below. 2. Unstratified yellow clay, containing isolated angular patches of reddish sand. 3. Whitish sand with a few scattered pebbles, sometimes changing into reddish sand, like that of the patches in the clay above. 4. Yellowish brown clay ("red brick earth"), unstratified at top and graduating downwards into laminated calcareous clay.

led to modify his opinion respecting the relation of the deposits to the glacial period. I myself believe this clay to be the upper boulder clay, and the sand with pebbles below it to be the "middle glacial sands and gravels."

To trace the "red brick earth," 4 in section, down towards the lower boulder clay, I set some men to work, and had a shaft sunk—at the point marked B in general section—to a depth of 17 feet from the top of the surface soil, and obtained the section shown in Fig. 9. The most noticeable feature in this section is the thickening out of the false-bedded sands and gravels, their resemblance to the middle glacial series, and the absence of the "white brick earth," 7 in section. In a pit a little east of this, Prof. Prestwich and Mr. John Evans found a flint implement in the gravel bed, 3 in section.

I have now given all the facts at present known respecting the relation of these beds to the glacial period, and I proceed to the consideration of Prof. Prestwich's theoretical views, as shown in the general section, Fig. 3. In the first place, Prof. Prestwich identifies the boulder clay seen in the pit on the east side of the brook as the upper boulder clay. As I have already mentioned it in no respect resembles the clay seen in other sections above the false-bedded sands and gravel, and the existence of the middle glacial beds below this particular deposit is entirely theoretical. Prof. Prestwich makes these sands and gravel to pass under the brick clays; and I feel confident it will astonish many of those who appeal to this section, as proof of the post-glacial age of palæolithic man, to learn that they have never been seen in this position, and that their presence is an assumption

Fig 9

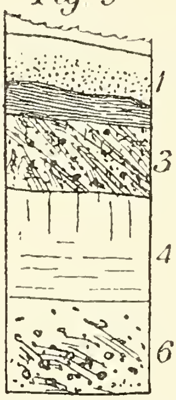


Fig. 9.—1. Sandy "trail" with flints: 3 feet. 3. False-bedded sand and subangular gravel: 4 feet 6 inches. 4. "Red brick earth," yellow and unstratified at top, graduating downwards into grey, laminated, calcareous clay; shells of *Bithinia tentaculata* and *Limnea palustris* abundant at its base, where there is about 6 inches of sandy clay: 4 feet 6 inches. 6'. Clay similar at top to the lower part of the "red brick earth," but with more chalk grains, gradually getting more chalky downwards, and with stones like the upper portion of the lower boulder clay at point A in general section.

only. The "red brick earth" ought, according to Prof. Prestwich's views, to thin out eastward, and the dark clays or "red brick earth" to crop up to the surface from underneath it. Instead of this, as shown in Fig. 8, at the point B in general section, the "red brick earth" follows down the slope of the hill, and is not underlaid at all at that point by the dark clays. I do not, however, attach much importance to this, as the "red brick earth" might mantle the hill, overlapping the edge of the dark clays, and yet Prof. Prestwich's general idea of the relation of the latter to the glacial beds be correct. What I do wish to point out is, that that relation is not proved by any of the facts known, and that an entirely different interpretation is not only possible,

but more probable. That other interpretation I have indicated in the general section, Fig. 5, in which all the facts observed are incorporated. I consider that the dark clay with vegetable remains and bones of the large extinct mammals is pre-glacial, in the sense that it is older than any of the glacial beds of the district. The gravel below the " red brick earth " in which Mr. Frere found the flint implements is probably of the same age, or of that of the overlying gravel, 5 in Figs. 4 and 5. That the implements, and also fragments of bones and wood, should be occasionally found in the overlying deposits, is what might be expected, as they were in great measure formed by the denudation of the older ones. The " red brick earth," 4 in section, is, I believe, a true glacial clay, belonging to the latter part of the first European lake. It is a noticeable fact that all over Northern Europe the glacial clays burn to a red colour,—a point not without significance with regard to the red beds of Permian or Triassic age. The false-bedded sands and gravels (3 in Figs. 4 and 5) belong, I think, to the middle glacial series, and the clay (2 in Figs. 4 and 5) is, I think, the upper boulder clay. These views are only theoretical, but I claim that they are based upon as sound a foundation, and are as much in accordance with the facts of the case, as those generally received.

Another interpretation is tenable, namely, that the lower boulder clay underlies the brick clays, and that the upper boulder clay overlies them, whilst they themselves belong to a warm interglacial period, as held by Messrs. Croll and Geikie. I do not agree with this opinion, as I can nowhere find any evidence of a warm interglacial period, and am unwilling to believe that there were more than one post-tertiary glacial periods, when one will explain all the phenomena; but if it were to turn out that the lower boulder clay does exist beneath the brick clays at Hoxne, it would be one of the strongest facts in its favour yet brought forward.

I now come to the real point and object of this paper. We have in England, at Hoxne, one of the finest opportunities known to exist anywhere in Europe of determining the true relation that the beds containing remains of palæolithic man and the great extinct Mammalia bear to the glacial period; yet we have been content for more than a dozen years to allow the age of the beds that underlie these deposits to remain a conjecture, and to accept a theory instead of ascertaining what are the true facts of the case. The geological world has been taught to believe that a question was settled that is not settled. We do know the

age of the Hoxne deposits: they may, as held by Prof. Prestwich, be post-glacial; or they may, as held by Messrs. Croll and Geikie, be inter-glacial; or, lastly, they may, as I hold, be pre-glacial.

It is not creditable that this uncertainty should remain when it can easily be cleared up. A few shafts or bore-holes put down would soon determine whether or not glacial beds underlie the dark clays of the brick-pit, or sands and gravel underlie the boulder clay on the other side of the brook. Excavations should also be made around the spot where Mr. Frere made his discoveries, to ascertain the exact position in which the flint implements were found so abundantly. I feel satisfied that if Sir Edward Kerrison, to whom the property belongs, were applied to by any of our learned societies, he would willingly allow the necessary excavations to be made. Probably the expenditure of two hundred pounds would be amply sufficient, and I submit that it is a work that should be undertaken by the Royal Society or the British Association, who make grants for scientific enquiry.

II. A SCHEME OF WATER-SUPPLY FOR VILLAGES, HAMLETS, AND COUNTRY PARISHES OF THE CENTRAL AND EASTERN COUNTIES.*

By Prof. Hull, M.A., F.R.S., F.G.S.,
Director of the Geological Survey of Ireland.

THAT the development of zymodic diseases in villages and rural parishes is due to "dirt,"—which, as Lord Palmerston defined it, is "matter in the wrong place,"—and also to bad water, is an axiom that is accepted as soon as stated.† Few, however, except those whose lives

* Read before Section G at the meeting of the British Association at Bristol.

† Such diseases as typhoid, enteric, or filth fever, diphtheria, diarrhœa, dysentery, &c., arise from the use of polluted water. See on this subject an able paper by Mr. Jabez Hogg, on "River Pollution" (Journ. SocArts, 1875).

are passed in such localities, are aware to how large an extent these preventible diseases are prevalent, even in places where beneficent Nature has granted all the necessary aids to health. The stagnant pool, the pestilential manure-heap, the odoriferous ditch, are but too frequently the immediate surroundings of a cottage or farmstead; and in the midst of these, or near thereto, is often planted the pump for the water-supply of the household, which, with the purer underground waters drawn from wider areas, again brings to the surface some proportion of the percolating filth.

When a deep well or a pump is absent, the water for household use is often drawn from still more objectionable sources. These may consist of shallow pools, hollowed out by the side of the lane or road, along which an intermittent stream of surface-water trickles, liable to receive natural or artificial impurities from the adjoining field or road. If the pool (dignified by the name of "well") be not in direct surface-communication with the ditch aforesaid, yet is it indirectly supplied by the percolation of the water through a few feet or inches of soil, which after a time becomes so thoroughly saturated with filth, that it ceases to act as a filter for the finer and less palpable varieties of noxious matter; and these, as is well known, are the causes of greatest injury to health, and the most fruitful sources of disease. When neither of the above means of water-supply are in use, recourse is sometimes had to the adjoining brook, supplying water of varying quality, but which is at present under no supervision for the prevention of the influx of matter of a noxious character. On the other hand, where the geological conditions permit, many villages and hamlets are supplied with water from perennial fountains or deep wells, which, drawing their waters from wide areas of strata through which the rainfall has percolated by the natural process, have thereby become suited for use. In the early planting of these islands such fountains were generally selected as the sites of hamlets or homesteads, and many of these remain to the present day along the borders of the Chalk, the Lower Greensand, and the Oolites; while within the area of the London basin—as Prof. Prestwich has pointed out—many of the suburban villages near the city of London were grouped around a spring of water, or bed of gravel from which water could easily be drawn by shallow wells.*

Before entering further upon the subject of this commu-

* Anniv. Address Geol. Soc. Lond., 1872.

nication, I wish to premise that I have no intention of dealing with the question of water-supply to large towns, or any of those places which would come under the head of "Urban Sanitary Districts" of the Public Health Act of 1872. Such districts and towns may well be relegated to the care of their respective sanitary authorities, guided by a corps of engineers, who are ever ready to find water if only the money for the purpose is forthcoming. It is to the more humble and greatly neglected villages, hamlets, and country parishes of the central counties my observations are intended to apply, with a view of showing that the means are generally available for providing them (where required) with that essential concomitant to health, decency, and comfort—good water.

Physical Considerations.

Though large portions of those districts of England referred to in this paper are destitute of hills,—the birth-places of springs and fountains,—yet it happens that the geological formations are arranged so as to constitute underground reservoirs for a large proportion of the ordinary rainfall, which can be rendered available by wells of greater or less depth. These districts are composed of the Mesozoic (or Secondary) formations, consisting of alternating beds of permeable, or water-bearing, and impermeable, or dry, strata; generally arranged with such a moderate dip as to spread over large areas, and capable of retaining on the one hand, or throwing off on the other, a certain proportion of rainfall, varying in amount according to circumstances.

The water-bearing strata consist of sandstones and limestones of various kinds; the impermeable, or non-water-bearing, of clays and shales; and it is therefore evident that any system of water-supply applicable in the one case would not be so in the other, and that the subject divides itself into two heads accordingly.

Let us, however, before proceeding further, take a rapid view of the formations referred to in descending order, commencing with the Lower Tertiary strata of the London Basin, which form the upper limit of our survey.*

* The supply of the London district and the water-bearing strata of the Thames Valley, which is beyond the scope of the present paper, has been admirably treated by Prof. Prestwich, in his Anniversary Address to the Geological Society of London (1872), to which the reader is referred. See also "Horizontal Wells," by J. Lucas, F.G.S., containing a remarkable and original scheme of water-supply, by intercepting the underground waters by tunnelling. Also, "Sixth Report of Commissioners on the Pollution of Rivers, and the observations thereon," by Mr. John Evans, F.R.S. (Quart. Journ. Geol. Soc., vol. xxxii., p. 115).

GEOLOGICAL FORMATIONS OF THE CENTRAL AND S.E. COUNTIES.

THICKNESS IN FEET.

Formations.	Permeable Strata.	Impermeable Strata.	Quality of Water.
1. *London Clay*	—	150 to 280	—
2. Lower Tertiary (sands and pebble beds)	70 to 100	—	Soft.
3. Chalk	645 to 1000	—	Hard.
3. Upper Greensand	100 to 400	—	Rather hard.
4. *Gault Clay*	—	130 to 200	—
5. Lower Greensand*	20 to 500	—	Soft & good.
6. Purbeck and Portland beds ..	0 to 60	—	Rather hard?
7. *Kimmeridge Clay*	—	300	—
8. Coral Rag and Grit	40	—	Rather hard.
9. *Oxford Clay*	—	350 to 400	—
10. Great and inferior Oolites ..	200 to 450	—	Hard.
10. Upper Lias Sands	20 to 200	—	Soft?
11. *Upper Lias Clay*	—	30 to 300	—
12. Marlstone, or Middle Lias ..	30 to 250	—	Rather hard?
13. *Lower Lias Clay*	—	500 to 600	—
13. *Keuper Marls* (Trias.)†	—	600 to 3000	Gen'ly imper.
14. Lower Keuper Sandstone‡ ..	150 to 450	—	Soft.
14. New Red Sandstone (Bunter)‖	0 to 2150	—	Soft or variable.
Lower Permian Beds (alternating characters)	Variable.	Variable.	Soft.

Thus out of the fourteen sets of strata arranged in the above table according to their water-bearing qualities, there are eight which are permeable, viz., (2) the Lower Tertiary Sands, (3) Chalk and Upper Greensand, (5) Lower Greensand, (6) Purbeck and Portland beds, (8) Coral Rag and Grit, (10) Oolites and Upper Lias Sands, (12) Middle Lias, (14) New Red Sandstone, with a total thickness varying from 1275 to 5600 feet; while there are, alternating with the above, six sets of strata which are impermeable, viz., (1) London Clay, (4) Gault Clay, (7) Kimmeridge Clay, (9) Oxford Clay, (11) Upper Lias Clay, (13) Lower Lias and Keuper Marls, with a total thickness varying from 2110 to 5030 feet vertical.§

* The Lower Greensand of Surrey consists of several members of varying hydrometric qualities, for an account of which see "Geology of the Straits of Dover," by W. TOPLEY, F.G.S., and "Horizontal Wells," by W. LUCAS, p. 21.

† The beds of Keuper Sandstone about the centre of the marls often contain water. In the Scarle boring, a feeder of water yielding 11 gallons per minute was struck in these beds, at a depth of 790 feet.

‡ In Scarle boring, about 250 feet thick.

‖ Scarle, 540 feet.

§ I have purposely omitted the clays and gravels of the drift-series, as they are of so variable a nature that no general rule can be laid down regarding their water-bearing qualities. The boulder clay is, however, a generally impermeable stratum, and the middle sands and gravels, water-bearing; from these springs of good soft water often issue forth.

Qualities of the Waters from the Permeable Strata.

In endeavouring to ascertain the qualities of the underground waters derived from different formations, it may be generally assumed that those drawn from limestone formations are "hard" and those from sandstone "soft." Owing, however, to variations in the nature of some of the strata in different localities, and to the greater or less proportion of carbonate of lime, carbonate of magnesia, or oxides of iron, &c., which they contain, the quality of the water from the same formation is liable to variation according to locality. This subject has already been so fully dealt with by various authors that I do not consider it necessary to do more than give a brief summary of the results as far as they have been ascertained in several localities.

Water from the Chalk.

From analyses of many wells and springs in this formation in the South-East of England, it is well known that "Chalk water" is hard, though clear and well-suited for many purposes, especially in the important one of brewing. The percolation of the rain through this formation, amounting to about one-third of the actual rainfall, is so exceedingly slow that the water has abundant time to take up a large proportion of carbonate of lime from the rock itself. The mode of percolation has been ably treated by Prof. Prestwich,* Mr. Clutterbuck,† Mr. Homersham, C.E.,‡ and others. It seems, from observations made on the Chalk hills by Mr. Beardmore,|| that it takes from four to six months for the rain to reach a depth of 200 to 300 feet, so that the water which is drawn from this depth in summer belongs to the rainfall of the preceding winter. The total quantity of solid matter in chalk water varies from 31 to 32½ in 100,000 parts, of which from 16·4 to 21 parts consist of carbonate of lime. In the case of large works this mineral ingredient can be dealt with by the softening process invented by Dr. Clark, but for country villages there seems to be no plan of easy application for lessening the amount of calcareous matter except that of boiling, by which the hardness is reduced from 24·7 to 3·7 in extreme cases.

* Anniversary Address, 1872, p. 41.
† Proc. Inst. Civ. Engineers, 1842-3 and 1850.
‡ Report, Royal Commission on Water-Supply, 1869.
|| *Ibid.*, p. 204.

Upper Greensand.

The water from the Upper Greensand is probably a little less hard than that from the Chalk.

Lower Greensand.

The water from this formation is generally considered unexceptionable, and decidedly "soft." Samples taken from five localities give a mean result of 7·9 of solid matter in 100,000 parts of water. Mr. Bateman, C.E.,* speaks of the water from this formation in the basin of the Wey as being of the greatest purity; and, as the sands are generally loose and incoherent, they absorb nearly all the rain which falls on their surface, except that given off by evaporation or imbibed by vegetation.

Enormous quantities of water are absorbed by this formation, where, as in Surrey and Kent, it is largely developed; and to such an extent are they capable of being rendered available that Mr. Lucas proposes, by means of tunnelling, to render them available for the supply of London.† In Bedfordshire, Bucks, and Berks, as also in the S.W. of England, the formation attains large proportions, and contains bands of siliceous iron ore, which to some extent affects the water. Exposure to the air, however, causes the iron to be precipitated.

Oolite Limestones.

The waters from these formations are all more or less hard, yet less so than those from the Chalk, and of course where the source is the sandy strata which accompany the limestones of the Portland Oolite, the Great and Inferior Oolites, &c., the qualities of the water will be found to vary accordingly. Of the proportion of solid matter in the waters of the Oolites that found in the fine springs of South Cerney, near Cirencester, which rise along the line of a large fault, may be taken as a sample. The total amount of solid matter was found to be 18 grains per gallon, of which 1·25 was organic.‡

The water from the Seven Springs near Cheltenham, from the Inferior Oolite, gave 6 grains per gallon, of which 2 grains consisted of organic matter.‖

* Report contained in a Return to an Order of the House of Lords, 1852, No. 258.

† Horizontal Wells, by J. Lucas, F.G.S. (1874).

‡ Analysis, by J. Horsley, F.C.S. Appended to Report on the Water-Supply of Cheltenham, by Dr. Wright, F.G.S.

‖ *Ibid.* See, also, Prof. Prestwich "On the Geological Conditions affecting the Water-supply of Houses, &c." (Parker and Co., Oxford), 1876.

The well at Thames Head, pumping from the Great Oolite near Cirencester, gave 16 grains per gallon;* and the waters of the Chelt, above Charlton Mill, near Cheltenham, which rise from springs at the base of the Inferior Oolite, gave 20 grains per gallon, of which 4 grains consisted of organic matter.*

Marlstone, or Middle Lias.

I have no analysis of water from this formation; but, judging by the appearance of the numerous springs which issue forth from the flanks of the hills of Marlstone in the Midland Counties, it may be considered of good quality and moderately soft. In a formation so variable as the Marlstone—consisting in some places of calcareous sandstones, in others of ferruginous limestones passing into ironstone—the character of the water must be liable to considerable variation in different localities.

New Red Sandstone.

Next to the Chalk, the New Red Sandstone—including the Bunter and Lower Keuper divisions—is the most important water-bearing formation in the district under consideration, and the water which it yields possesses this advantage over that of the Chalk, that it is softer, and generally capable of being used for all domestic and manufacturing purposes. From the numerous analyses that have been made of these waters in different localities in the central and N.W. counties, we have the means of arriving at general conclusions on this subject, and for special and accurate details we may look forward with interest to the Reports of the Committee on Underground Waters, appointed by the British Association in 1874.† The beds of the Bunter Sandstone are wonderfully adapted both to act as natural filters and as reservoirs for that portion of the rain which sinks below the surface. This may be assumed at one-third of the actual rainfall as an average; while in some districts—where the formation consists of soft sandstone or unconsolidated conglomerate, devoid of a thick covering of drift clay—the amount of absorption must reach well nigh one-half the amount of rainfall. Owing, also, to its uniformity in composition, and the absence of beds of clay or marl of any importance, the whole mass of rock below a certain level, and throughout a depth of several hundred feet in some districts, becomes

* Horsley's Analysis, *sup. cit.*, p. 309.

† Belfast Meeting. First Report presented at Bristol, 1875.

water-logged, and wells sunk therein do not (as in the case of the Chalk) generally depend for their supply on the presence of fissures, but water is nearly always found after the "water-level" of the immediate district has been reached. This was the view, since abundantly confirmed, taken by the late distinguished engineer Mr. Robert Stephenson, in his "Report on the Water-Supply of Liverpool," and has been acted upon with success by several engineers who have sunk wells for the supply of Liverpool, Manchester, Birmingham, Nottingham, and other towns in the northern and central counties.

The remarkable permeability of this formation, equalling probably that of the Lower Greensand both of England and France, has recently been illustrated by the deep boring in search of coal at Scarle, near Lincoln. This boring, conducted by the "Diamond Boring Company," was commenced in the Lower Lias in 1874, and, after having passed through 790 feet of these beds and the underlying impermeable marls of the Keuper, struck a feeder of water in the Lower Keuper Sandstone, and a still stronger one at 950 feet, which caused the water to spout up 5 feet above the surface. The whole of the Bunter Sandstone below was also charged with water, as proved by the increasing temperature down to its base of 1500 from the surface.* Now, the distance of this boring from the outcrop of the beds towards Mansfield varies from 12 to 16 miles, and through this distance the water percolated from a district 300 or 400 feet above the sea-level, till it reached a depth of about 900 feet below it; and, being kept down by the overlying impervious beds of the Keuper Marl, ascended to the surface with great force on being released through means of the borehole on the Artesian principle.

The amount of solid matter per gallon in the waters of the New Red Sandstone varies from 6 to 15 grains where they have been taken from wells not too shallow, or from those which are free from contamination by sewage pollution. It is to such a cause that the large proportion of saline and other ingredients in some of the Liverpool and Manchester wells, amounting in some instances to 24 and 36 grains per gallon respectively, is attributable. In general, the proportion of these ingredients occupies a central position between those of the Chalk and other limestone formations on the one hand, and the surface-waters of mountain

* From information kindly afforded by Mr. J. T. Boot, mining engineer, under whose superintendence the works were carried forward.

districts composed of millstone grit or of Silurian rocks on the other. The following examples will probably be considered sufficient for the purpose here intended :—

	Wells or Springs.	Grains per Gallon.
Liverpool District* . .	Bootle	24·00
,, ,, . .	Soho	24·80
,, ,, . .	Windsor.	23·22
,, ,, . .	Green Lane . . .	13·60
Birmingham†	Aston	12·82
Stourbridge‡	Well of W. W. Co. .	21·95
Leek (Staffordshire)‖ .	Wall Grange Spring.	12·26
Whitmore (near Crewe)§	Well of L. & N. W. Ry.	6·10
Parkside (nr. Warrington)¶	Well	11·12
Nottingham	Several wells (suburban) . . .	12·0 to 16·0

As an illustration of the effect of the percolation of saline matters on the waters of even deep wells in large towns, I give here the analysis of the waters of a well from the south side of Manchester, made in 1865, by Dr. Angus Smith, F.R.S. This will show the desirability of having all wells as far as possible removed from such sources of contamination.

Water from Well, Manchester, South Side.

	Grains per Gallon.
Chloride of sodium	4·88
Sulphate of soda	7·33
Carbonate of soda	7·35
,, of lime	9·77
,, of magnesia	5·29
Phosphoric acid, potash	traces
	34·65

Lithia discovered by spectroscope.

On the other hand, the purity of the waters from the Aston Well, the Wall Grange Spring (yielding 3,000,000

* Analyses by Richard Phillips, F.C.S.
† Analysis by Dr. Hill, F.C.S.
‡ Supplied by Mr. E. Bindon Marten. This well is only 46 feet deep.
‖ Analyses by R. Phillips.
§ This well supplies the works and populace of Crewe, and was sunk, on the recommendation of the author, by the London and North-Western Railway Company, 1864.
¶ Analysis by Dugald Campbell, F.C.S.

gallons per day), the Whitmore, Parkside, and Nottingham Wells,—which are removed from the influence of populous neighbourhoods,—must be considered as completely establishing the excellence of this source of water-supply.

Considering, therefore, the wide extension of the water-bearing strata of the New Red Sandstone, the excellence of the formation as a source of pure water,—equally cool and refreshing in summer and winter,—it must be conceded that the central counties are happily situated as regards their prospects of water-supply, provided these resources are judiciously utilised. Nor must it be forgotten that the internal reservoirs do not terminate with the upper boundary of the sandstone, but that the waters pass for great distances under the overlying impervious marls, as illustrated by the case of the Scarle boring, and that, by deep wells or borings, these underground reservoirs may be converted into perennial fountains.*

Application of the above Observations to the Cases of Villages and Hamlets.

From what has been stated it will be obvious that for those villages, hamlets, or parishes situated on any of these water-bearing strata, or not far from their *upper margins*, where they pass below the overlying impermeable formations, wells are the proper and available means of supply. But the determination of the question regarding the character of the formation underlying each special village or hamlet, as well as the selection of the best site for a well, are evidently geological questions, requiring some special knowledge of this branch of science; and the problem how such knowledge is to become available in each case remains for solution. To this I now address myself.

In the first place it will be admitted that the completion of the maps of the Government Geological Survey, over the districts comprised in this paper, places them in a very advantageous position for availing themselves of the underground waters stored up in the strata. Until some accurate and detailed maps such as these were rendered available, the data for arriving at definite conclusions regarding the geological situation of many localities could not be obtained; and constituted sanitary bodies might well have hesitated to

* Thus at Retford, Notts, which stands on the upper beds of the Keuper marl, not far below the base of the Lias, there are two wells sunk down 600 feet into the New Red Sandstone for the supply of two large breweries, and the water rises within 6 feet of the surface. One of these wells was sunk under the direction of Mr. John T. Woodhouse, F.G.S., of Derby.

accept advice from amateurs, of whose mode of observation and process of reasoning they were generally ignorant. But the maps of the Geological Survey, published under the authority of a Public Department, and showing the limits of the water-bearing and impervious formations laid down with the greatest attainable accuracy, furnish ready to hand all the data for determining the preliminary questions. With these maps a Sanitary Board ought to be able, with very little assistance of a technical kind, to determine whether a particular village or hamlet, requiring good water, was so placed geologically as to be able to obtain a supply by means of a well; and, if so, also the best spot in which such a well should be sunk.

I am aware, however, of the unhappy and prevalent ignorance of even the most rudimentary principles of geology amongst such bodies, and it would therefore be necessary—for the proper carrying out of the proposals here suggested—that an experienced geologist should be attached to the staff of the Central Board of Health in London, whose duty it should be, when applied to, to afford advice in all cases of this kind. Owing, therefore, to the progress of the Survey over the districts here alluded to, it seems to me that the time has arrived for putting in force some scheme of general application depending on geological considerations.

Cases not admitting of Supply by Means of Wells.

I now come to the case of those villages, hamlets, or parishes which, owing to their geological position, cannot be supplied by means of wells. These are they which are situated on such impermeable strata as the Oxford Clay, the Lias or the Keuper Marls, or are at such a distance from the subjacent water-bearing strata that the depth of a well would be great, and the expense too costly for the resources of the inhabitants. In such cases a supply from surface drainage of some available brook or rivulet seems the only resource; and powers should be given by the local sanitary authority to impound such stream in small reservoirs, under proper and stringent regulations, so as to prevent surface pollution of the water either in the stream or in the reservoirs. The construction of such small reservoirs as would be necessary for the purposes here alluded to would entail only a moderate amount of engineering skill and of pecuniary outlay. In providing pure water it would also be necessary to stop up or destroy all impure wells, pools, or tanks, in the same village or hamlet, as the poor are very

apt to prefer a source of supply to which they have all along been accustomed.*

Machinery for carrying out a General System of Water-Supply for Villages and Hamlets.

We come now to the difficult question as to the machinery for carrying out such a system of water-supply as is here proposed ; and perhaps the first step to be taken is to ascertain to what extent the evil of insufficient or polluted water-supply prevails in country places.

This, I think, might be effected by means of the agency evoked under "The Public Health Act of 1872." Under the clauses of that Act the Rural Sanitary Authorities have the power of appointing a Medical Officer of Health and an Inspector of Nuisances for a period of five years; and it would probably not be difficult, by their co-operation, to obtain returns from all the parishes included in the rural sanitary districts, together with the villages and hamlets contained therein. I would propose that a simple form of circular be drawn up by the authority of the Local Government Board, which the officers above named should be required to fill up and return, stating—1st. The names of the villages or hamlets in each rural sanitary district. 2nd. The number of houses. 3rd. The existing mode of water-supply. 4th. The opinion of the medical officers regarding the nature and quality of such water, if any there be. And 5th. Observations regarding the general health of the inhabitants, and the presence of zymodic diseases.

It would then be the duty of the Local Government Board to eliminate the names of those villages or hamlets in which it would appear, from such returns, that the water-supply was sufficient, both as regards quantity and quality; and then would remain those residuary cases requiring to be dealt with under the scheme here proposed. They would probably be found to amount to several hundreds within the limits of the area dealt with in this paper.

It would doubtless be advisable, in carrying out such a scheme, that the existing parliamentary powers should be called into requisition. After perusing "The Public Health Act of 1872," it seems to me that with slight modification that Act might be made available. The division of the whole country into (1) Urban and (2) Rural Sanitary Districts is excellent, and at once determines the districts

* Such power is granted to the Local Sanitary Authority under the Act of 1874.

outside, and within, the scope of the proposals I have ventured to offer. The appointment of Medical Officers of Health by the Sanitary Authorities, but who are responsible to the Central Authority in London, offers an effective agency. Under Clause 17 the Rural Sanitary Authority has power to provide a supply of water for "Rural Sanitary Districts," but such districts are, I fear, too extensive for such modes of water-supply as is here proposed, and it would be necessary to split them up into villages and hamlets, having independent powers, though the necessary expense might be spread over each district.

I would therefore propose that when it is found, from the Reports obtained through the Rural Sanitary Officers, that the supply of water to any village or hamlet is insufficient or impure, the Sanitary Authority of the district in which the village or hamlet is situated should be required to proceed forthwith to apply the remedy. It would be necessary then to have recourse to the Geological Survey maps to ascertain whether the locality could be supplied by a well, and if the local technical acquirements are insufficient to settle this question, the aid of such a Government officer as has been already referred to should be afforded; while at the same time it should be competent for the Sanitary Authority to call in professional advice from any other quarter.

A well having been determined upon when the geological conditions prove favourable, the site fixed with reference to local circumstances, and the cost having been ascertained, powers should be granted to raise money for carrying out the work and rendering the well available for the free use of the whole of the inhabitants.

In sinking such a well, the top waters—to a depth of 10 or 12 feet—should always be carefully stopped off by solid masonry; and means be adopted for keeping it free from contamination, under stringent regulations and heavy penalties. Being for the use and benefit of all, it should be jealously guarded by all as a public benefactor.

I have not considered it necessary to enter at length into the means by which my proposals should be carried out. These are matters of detail, capable of easy settlement by those who are conversant with such matters. I have contented myself with sketching out a plan of general application, which will probably be considered sufficient for the present occasion.

By one or other of the above methods probably all the villages or hamlets of the Central and Eastern Counties which require it might be supplied with pure water. Till

this is done it cannot be considered that we have reached the state of sanitary improvement befitting a great and prosperous country.

Postscript.—Since the above was written I have had an opportunity, through the courtesy of Mr. Sclater-Booth, M.P., the President of the Local Government Board, of perusing a copy of the Public Health Act of 1874, which appears to me admirably calculated to meet the case of the districts referred to in this paper, and to provide the machinery by which the suggestions here offered might be carried out.—E. H.

III. VIVISECTION.

MYSTERIOUS and perplexing are the ways of modern Humanitarianism. One moment it swallows the largest of camels, and the next it strains at the tiniest of gnats. With unfaltering step it marches over the summit of Mount Everest, and then stumbles at some scarcely visible mole-hill. To drop metaphor, it fastens upon matters never demonstrated to be evils, minute in their amount and unobtrusive in their character, whilst practices much more questionable in their nature, and almost world-wide in their extent, are allowed to pass unchallenged. Perhaps, however, there is method in this seeming madness. Sport and Fashion are popular and powerful, and may therefore—to quote a somewhat cynical proverb—" steal a horse" with impunity, whilst Science, weak and uninfluential, " is hanged for looking over the gate." This may perhaps, in some degree, explain the recent raid made upon vivisection. But if Humanitarianism is cautious as to whom or what it attacks, when it has once selected a victim it is equally pertinacious and unscrupulous. It never retracts, never owns itself mistaken. We have heard that there is no animal, not even a *Galeodes*, so aggressively pugnacious as an advocate of " international arbitration ;" none so narrow-minded and selfish as the professed cosmopolitan ; and, in like manner, it might be said that of all enemies the most bitter, vindictive, and unchivalrous is the humanitarian. There is with him no toleration, no agreement to differ. " Be my

brother or I must kill you" is his motto, and too often, if unable to overcome his opponents in argument, he assails their private characters. Now when "world-betterers" combat any real evil—of which there is a tempting assortment awaiting their attention—this fanatical zeal may sometimes do good service; but when they seek to fetter the development of Science, and thereby to check the progress of the world, they must be encountered by a determination as stern as their own. Whilst scorning to borrow their unfair weapons, and whilst giving them full credit for sincerity of a certain kind,—a credit which they have to share with Thugs and Inquisitors,—we must refuse to concede even a hair's-breadth of their demands. We know that certain accusations are supposed to require no evidence, and that to defend the accused is an invidious task. Such was in former ages the case with charges of heresy and witchcraft; so it is now with charges of cruelty to animals. Yet though certain to be misrepresented, we must proclaim the outcry raised against vivisection to be the outcome of misconception and inconsistency.

We will first examine the cases in which man claims the right to inflict pain upon the lower animals. Of these there seem to be five classes, namely—

1. In self-defence.
2. In order to obtain articles of real or supposed necessity.
3. In compelling animals to obey his will.
4. In the pursuit of amusement.
5. In the pursuit of knowledge.

The pain thus inflicted may vary in degree from slight uneasiness, or from a momentary shock, to intense and prolonged torture. It includes terror, distress, and exhaustion, as well as the direct action of mechanical or chemical agents upon the victim. It includes death, for—except by the use of certain narcotic poisons—death cannot be induced without some kind of pain. These different cases we will now consider, enquiring if, and in how far, man is justified in the line of conduct which he generally, if not universally, adopts towards his "poor relations." We shall not seek to cut asunder the knot by the assumption that animals, even the lowest, are utterly incapable of feeling,* or by declaring, with Prof. Mivart, that, though sentient, they are uncon-

* Lamarck grouped together certain of the lower forms of life as "*animaux apathiques*." We doubt whether even vegetables are absolutely incapable of feeling.

scious of their feelings, like a man under the influence of chloroform. On the contrary we will, for the present, take it as proven that animals both feel and know that they feel.

We turn to our first case, the infliction of death, and consequently of pain, in defence of our persons and property. In virtue of that struggle for existence which, whether "Darwinism" be true or false, is an acknowledged fact, one species can only exist at the cost of others. There are beings—too many to be here enumerated—which, directly or indirectly, seek to make man their prey. From the lion, the tiger, the cobra, or the shark, down to the flea, the mosquito, the *Lucilia hominivora*,* the *berne*, the tape-worm, the *Trichina*, and even to those microscopic beings which are now believed to be the propagators of pestilence, we are attacked in our persons by an incalculable horde of enemies. At the same time we have indirect assailants scarcely less formidable. The rat, the mouse, the locust, the mole-cricket, the potato-beetle, weevils, flies, and slugs of many kinds, seek our destruction by the process of destroying our means of subsistence. We have, therefore, simply the alternative placed before us to kill or to be killed. We have so far—with the exception of certain Oriental fanatics—elected the former alternative, and with bullets, traps, poisons, medicines, and appliances of the most varied kinds, we wage war alike against wild beasts, vermin, Entozoa, and disease-germs. So far our humanitarian zealots have discovered no wickedness in this struggle, and have not yet proposed to substitute "arbitration" as a means of dealing with "man-eaters." This, we presume, is a pleasure held in reserve for posterity.

We may therefore assume that the right to exist involving the right to kill, and it being generally impracticable to kill

* Van Beneden's "Animal Parasites and Messmates"—a work which might have been expressly written as the *reductio ad absurdum* of the old Natural History—contains the following notice of this detestable vermin;—"Vercammer, a military surgeon of the Belgian army, relates that a soldier in Mexico had his glottis destroyed, and the sides and roof of his mouth rendered rugged and torn, as if a cutting-punch had been driven into those organs. This soldier threw up with his spittle more than two hundred of the larvæ of this insect." Yet the author opens his subject with the following passage, conceived in the true spirit of Cuvier, or of the Bridgewater Treatises:—"In that great drama which we call Nature each animal plays its especial part, and He who has adjusted and regulated everything in its due order and proportion watches with as much care over the preservation of the most repulsive insect as over the young of the most brilliant bird. Each as it comes into the world thoroughly knows its part, and plays it the better because it is more free to obey the dictates of its instinct." The idea of a being whose "especial part," divinely adjusted and regulated, is to destroy the glottis of man and to punch holes in the roof and sides of his mouth, is somewhat suggestive.

without causing pain, man has the general right to inflict pain upon the lower animals where his well-being requires, the conditions being that such pain shall not be wantonly and unnecessarily inflicted, and shall be minimised both in its degree and duration.

The second case, the destruction of animal life in order to obtain desired articles, is in its extent inferior only to the one just mentioned, and much more heterogeneous in its nature. Man kills animals to subsist upon their flesh, and even the most rigid vegetarian must admit that in high latitudes he has no other option. Here, then, as in the first case, the infliction of pain is a matter of absolute necessity. In temperate and tropical climates the circumstances are different. Here vegetable food is procurable in sufficient quantity for the support of life. Still the experience of the vast majority of men leads them to believe—and to act upon the belief—that a diet composed in part, at least, of animal matter, is most conducive to their vigour, health, and comfort. Hence we find that, except vegetarians, all mankind believe themselves justified in inflicting death, and consequently pain, upon the lower animals, not merely at the biddings of absolute necessity, but at those of convenience; law and public opinion stipulating merely that no amount of pain shall be inflicted beyond what is clearly necessary to the object in view. This, as we shall further find below, is an important conclusion.

But man takes life for many other purposes than for mere food: he desires hides, horns, furs, feathers, ivory, animal fats and oils, and considers himself fully justified in satisfying these desires, however extreme or whimsical, by the destruction of life. The savage, in need of clothing and unable to manufacture woollen garments, may indeed plead the necessity of wrapping himself in furs; but can civilised man, who is well acquainted with the art of producing artificial coverings equal if not superior to furs, advance the same plea? All that he can say in justification of his practice of killing and torturing, in order to obtain furs and feathers, is to proclaim that not his necessities, not his convenience or well-being, but his luxuries, whims, and caprices are a full warrant. It may be here useful to glance at the seal-skin trade as an instance in point. Till a comparatively recent period seals were hunted merely for their flesh and blubber, by Greenlanders, Esquimaux, and occasionally by whale-fishers from civilised nations: their skins were in little demand, being unattractive in colour. Unfortunately for the seal, and it may be added unfortunately for the

honour of humanity, a method was discovered of converting the greyish hue of its fur into a rich lustrous brown. Forthwith seal-skins became the rage, not for the caps or waistcoats of sailors and fishermen, or the garments of Skrælings, but for the jackets of fashionable ladies, and found a ready sale at high prices. To obtain them extensive hunting expeditions were sent out, and conducted with an amount of cruelty which is perhaps without a parallel in all the dealings of man towards the lower animals. Seals are most readily captured at the time when they have young cubs not yet capable of following their mothers through the water. At this time they may be found upon the shores of certain arctic regions in great numbers, and here accordingly they are attacked. The mother seals are stunned with blows from clubs, and then flayed, often before quite dead, it being considered that the fur is thus obtained in a more lustrous condition. As for the young ones, they are left to perish of cold and hunger. The frightful atrocity of this system will be more fully understood if we remember that the seal stands high in the scale of animal life, and possesses a large well-developed brain and a delicate nervous system. All this cruelty is therefore done for the sake of "fashion," and to it all wearers of seal-skin jackets make themselves accessories. It is true that some voices, in England and elsewhere, have been raised against this system, and that some attempts have been made to mitigate its horrors by legislative enactments. But policemen, officers of the Society for the Prevention of Cruelty to Animals, and magistrates, cannot follow every seal-hunting expedition to the northern seas; and there is every reason to fear that as long as the demand for seal-skin jackets continues, the supply will be obtained substantially in the manner we have sketched.

Feather beds are no longer, as was once the case, considered preferable to all others. But they are still in extensive use, and live goose feathers—*i.e.*, such as are plucked from the unfortunate birds whilst still living—are still preferred as being more elastic than those obtained after death. Whether this practice is legal at the present day we know not. But a demand exists for such feathers, and they are still advertised for sale, no one denouncing such needless cruelty.

Somewhat similar must be our conclusion concerning the present mania for using portions of birds, or even entire birds, as ornaments. In consequence of this whim rare and beautiful birds, trogons, sun-birds, humming-birds, birds of paradise, and others, are shot down at random, and are imported not as

formerly, in occasional specimens for museums, but literally in heaps. We do not mean to say that these birds are subjected to such prolonged tortures as are the seals. They are shot with the gun or the blowpipe ; but their nestlings are often left to die of hunger, and, above all, their lives are taken wantonly and needlessly. That most of the species so persecuted are insect-feeders, and consequently the friends of man, is an aggravation of the case. We love consistency, and therefore feel bound to record the fact that one of the foremost agitators against vivisection has also denounced the practice of using birds as materials for the milliner and the *modiste*. Summing up the second case, we may say that whenever man finds any portion of the body of an animal saleable he thinks himself justified in procuring it by the infliction of pain and death, with little regard either to the urgency and importance of the demand thus gratified or to the exact nature of the means employed in obtaining a supply. The buyer, on his part, seems fully satisfied that his whims are of paramount importance.

We pass next to the third case. Man makes use of the services of certain domestic animals, and in training them for various purposes, and afterwards in the execution of such purposes he inflicts upon them a considerable amount of pain. It is not to be expected that horses, asses, camels, &c., will of their own accord carry burdens or draw weights. Man, therefore, has recourse to compulsion. Here, as in the former cases, the pain inflicted varies very greatly in degree, which of course does not affect the principle involved. We must here point out that though the services of such animals are a very great convenience, they are not absolutely necessary. Mexico, before the Spanish conquest, was an instance of a civilisation without beasts of burden. Hence, therefore, all persons who—whether directly or indirectly, whether habitually or occasionally—make use of beasts of burden, declare, in fact, that they are justified in inflicting pain upon animals whenever it may suit their convenience. Under this case must also be included painful operations practised upon brutes to render them more subservient to man, more fit for his purposes, or more in harmony with his caprice. The docking to which the tails of horses were subjected in the earlier part of the present century is an instance of this nature. Here, too, belongs the use of the bearing-rein, condemned indeed by a vast majority of those who claim to speak with authority upon such topics, but still very widely practised, and not likely to meet with legislative interference.

Man, in the fourth place, inflicts death and pain, bodily or mental, as a mere pastime. So common is this that, even amongst a people so ostentatiously virtuous as ourselves, more perhaps than among nations who make less boast of moral pre-eminence, "amusement," or its synonym "sport," is supposed to involve almost of necessity the taking of life. It is perfectly true that many of the pastimes of former ages which turned solely upon man's love of inflicting or of witnessing pain have been abolished. Gladiatorial shows, combats of wild beasts, bull and bear baiting, and pugilistic encounters are no longer tolerated; but cock-fighting seems to be experiencing a revival, the offenders, when detected, meeting with but very trifling punishment. The distinction, moreover, between what is punished as cruelty and what is tolerated—or rather applauded—as "sport" is at times very evanescent. Thus to course hares or rabbits in an enclosed place is "baiting," and as such subjects all concerned in it to the correction of the law. To do the very same thing in an open tract of country is "sport," and involves neither legal nor social penalties. Yet the only difference is that in the former instance the animals pursued have some small chance of escape. But that their terror is less when fleeing, or their sufferings smaller when caught, no one can maintain.

It is, indeed, contended on behalf of the chase, as practised in England,—*i.e.*, where an animal is hunted not in order to get rid of a nuisance or a danger, nor for the sake of obtaining its flesh or other valuable portions for use—that the real object is not so much amusement as health and the cultivation of certain manly virtues which a free nation cannot afford to let die out. We are willing to accept this plea in the utmost reasonable extent. Still we may ask whether health might not be as well secured by botanical, geological, artistic, or antiquarian rambles in moorland and forest as in shooting or fishing expeditions? The question might further be put—whether the manly virtues referred to may not be quite as advantageously cultivated in the destruction of beasts really dangerous to man, and which are still to be found in plenty, if not in the home kingdoms at least in other provinces of the empire? But whilst we freely admit that courage, endurance, hardihood, are required in and developed by fox-hunting, deer-stalking, grouse-shooting, and the like, can this be said in favour of the prosaic massacres of sparrows, pigeons, and half-tame pheasants? Were we not aware that inconsistency is one of the most striking attributes of human nature we should declare that the nation which can tolerate Hurlingham, and

yet raise an outcry against vivisection, must be the very focus of hypocrisy.

We now come to the last case. A small amount of life is sacrificed and a small amount of pain is inflicted in the pursuit of knowledge. Singularly enough this occasions a greater outcry than any of the former cases. We come, then, to the singular conclusion that it is permissible to kill animals, and to inflict pain upon them, for our safety, for emolument, convenience, luxury, ostentation, or amusement, but not for the acquisition of knowledge! Surely such a proposition, if once fairly stated, carries with it its own refutation. But the knowledge sought for and acquired by means of vivisection is not for the mere gratification of our curiosity: it has already thrown most valuable light on the preservation of health, the prevention of disease, and the consequent prolongation of life. Hence it may be said that the animals submitted to vivisection are indirectly, indeed, but not the less decidedly, sacrificed to our safety. If we may not destroy life for our safety, what right have we to take it for any purpose less urgent? Therefore we maintain that no person who kills animals, or causes them to be killed or subjected to pain for any purpose whatever, or derives any benefit from such death or infliction of pain, is logically justified in denouncing biological experiments. But some opponents may urge that what they object to is not the mere death of a few animals, but the excessive and prolonged torture to which they are previously exposed. We reply that if it is once conceded that we may under certain circumstances inflict pain upon animals, provided that we have a worthy and important end in view, the precise amount of pain is no longer a question of principle. Surely for an end so important we may take means which would be unjustifiable if our purpose were merely to compel a refractory beast to obey our will or to minister to a frivolous passion for ostentation and display. The pain inflicted in vivisection is much smaller in the number of cases, and certainly not more intense or prolonged in any one case, than that to which animals are subjected for other and less important ends. We may be told that one wrong does not justify another; but may we not bid the agitators take the beam out of their own eye before they seek to remove the mote from ours. When anglers and coursers, *battue*-sportsmen, Hurlingham-heroes, and wearers of seal-skin jackets presume to denounce vivisection, their only right is that of impudence.

It must be remembered that biologists, far from inflicting

pain needlessly and wantonly, or from taking any pleasure in its infliction, seek, by every device conceivable, to minimise the sufferings of the animals operated upon. The smaller the shock given to the subject, and the less its normal condition is affected in anything beyond the exact point at issue, the more trustworthy will be the result. The biologist, therefore, who should inflict any needless or avoidable torment on the animals experimented upon, would simply defeat his own object. It is important here to remember that, according to the Report of the "Royal Commission on Vivisection," the Secretary of the Society for the Prevention of Cruelty to Animals frankly acknowledges that he does not know a single case of wanton cruelty. Surely if such an admission is made by the responsible official of a powerful and influential organisation, with abundant means at command for probing the matter to the very bottom, and spurred on to action by a sensational outcry, we shall be warranted in assuming that no such cases exist.

Is it, then, rational to attack what is thus confessed to be free from "wanton cruelty," so long as in other directions so much wanton cruelty exists?

It is urged that vivisection must have a degrading and brutalising effect on all who are engaged in it. Such a result, they urge, would be especially deplorable in the case of medical practitioners, who should be eminently humane and sympathetic, if their services are to be acceptable to the suffering.

Had we to deal with ordinary opponents we should challenge them to produce an instance of the "degradation" of which they speak. We should point to illustrious men, living and dead, who have enlarged the territory of biological science and the resources of medical art by experiments upon living animals, and should ask them if the humanity of these men had been or could be called in question? But we fear that the anti-vivisectionists would, in answer to our challenge, "evolve out of their own consciousness" charges of cruelty. Such calumnies would, indeed, ultimately find their level, but we cannot make ourselves a party to carrying the war into the sacred sphere of private life.

There is, of course, a surface plausibility in the notion that the practice of vivisection must render men generally inhuman; but on closer examination it fades away. The biological experimentalist inflicts pain upon animals for one important end only, regarding it all the time as an unpleasant necessity. Where this end does not come into view he has no temptation to use the means.

It may be said that men can by habit become blunted to the exact nature of what they do, see, or experience. This is true, but the blunting process is special, not general. A man long inured to some particular form of danger, and grown indifferent to that one form, may be nervous, or even timid, if suddenly exposed to some novel peril. We have heard of reckless miners—accustomed to open their safety lamps in a fiery vein, and to calmly smoke their pipes over a keg of gunpowder—displaying no small trepidation if out at sea in a moderate breeze. Precisely in the same manner men accustomed to inflict pain or death upon one particular class of animals are not necessarily, on that account, blunted to the sufferings of other brutes, and still less of their own species. Fishermen, butchers,* poulterers, are by no means more inhuman or ferocious than other persons of similar position in life and degree of culture. Sportsmen and anglers cannot, certainly, as a class be accused of general and habitual brutality. Some of them are even zealous and distinguished philanthropists. Why, then, should it be asserted, without a tittle of evidence, that medical men who have practised vivisection, or who have made any experiments upon living animals, must be rendered callous to human suffering? Inflicting pain, as they do, more rarely than the classes of men above-mentioned, and for the highest purposes only, it is extremely unlikely that such a result should ensue; and in the absence of affirmative facts, which had they existed would have been trumpeted through the length and breadth of the land, we must dismiss this argument as idle and frivolous in the extreme.

In proof that great respect for animal life does not necessarily involve reverence for the superior sanctity of human life, we may refer to the sect of the Jains, generally supposed to be an offshoot of the Buddhists, and still numerous in some districts of India. Some of their priests never sit down without previously sweeping the spot, lest they might crush out the life of some creeping thing. They refuse to eat in the dark, as they might possibly swallow an insect, and thus destroy its life. "To prevent their inhaling an animalcule, the more rigid of them wear a thin cloth over their mouths." The pinjaropol, or hospital for decayed animals, maintained in the city of Surat, finds amongst them

* In proof that the habit of destroying animals of one particular species does not render a man generally indifferent to animal suffering, we may mention a case for which we can vouch:—A butcher discovered that a hen in his possession had contracted the habit of eating her own eggs. According to the laws of the poultry yard this offence is capital; but the butcher experienced a reluctance to kill the bird which he could not overcome, and he was obliged to send for a poulterer to perform the execution.

enthusiastic supporters. But though they consider themselves on this account far superior to Christians in a moral point of view, the life of man is no more respected amongst them than it is amongst people who do not evince such exaggerated tenderness for the brute creation. We cannot refrain from quoting a short and significant discussion between a Jain priest and an English missionary;—

"You take life," said the Jain; "we take none."

"Oh! what a happy country yours must be," replied the missionary: "you have no wars——"

"We have wars," said the Jain, interrupting.

"I thought you said that your people never took life?" returned the missionary.

"We take only human life," said the Jain.

In fact, India has, generally speaking, shown that an overstrained tenderness for animals may coexist with great cruelty to man.* Southern Europe, on the contrary, has been consistently merciless to both man and beast, and has tortured heretics, Moors, Jews, bulls, and horses with the utmost impartiality. We see, therefore, that the utmost tenderness to brutes may be accompanied with cruelty towards men, just as, conversely, a habit of inflicting pain upon animals for certain purposes and on especial occasions may coexist with kindness to man, and even to the lower animals where such purposes are not involved. All this may, no doubt, be grossly inconsistent conduct, but inconsistency is one of the most cherished privileges of human nature, and anti-vivisectionists should be the last to complain of it. The real germ of cruelty, of which we all ought to beware, is the infliction of pain for frivolous, unworthy, or impossible ends, and especially the making it a source of gratification. From all this there is no one more remote than is the vivisectionist.

The agitators urge that vivisection is not a trustworthy method of interrogating nature. This objection, if well founded, would be decisive, since in that case all operations undertaken upon living animals would be mere gratuitous inflictions of pain, and would then be justly branded as cruelty; but, being unfounded, it is remarkable chiefly for its impudence. Might we not, in common charity, suppose that the distinguished physiologists of the past and the present who have from time to time undertaken experiments

* The Turks are famed for their humanity to animals. What other nation, for instance, would not long ago have made a clean sweep of the loathsome dogs which infest the streets of Constantinople and of other large cities of the Ottoman Empire? Yet to man they have been merciless.

upon living creatures would be able to judge whether this method was safe or fallacious? Do they really require to be instructed on this head by unscientific outsiders? Can we really assume that physiologists would for one moment have recourse to a means so disagreeable if they could either discover a substitute, or if, on checking and verifying their results, they found them essentially delusive? The process is difficult, but so is all experimental enquiry; and few methods in any science have, in judicious hands, yielded a more splendid harvest. A medical contemporary justly remarks that "the whole history of medicine is pregnant with examples of benefits to humanity derived from such experiments."

The discoveries of the action of the lacteal and lymphatic system, and of the compound function of the spinal nerves may serve as illustrations. These discoveries, it has been justly said, lie at the very foundation of our present knowledge of the laws of animal life. Yet these discoveries are mainly due to vivisection. Turning from abstract biology to practical medicine, what would be our treatment of the diseases of the heart and blood-vessels if we suppose Harvey's great discovery blotted out? Hunter's treatment of aneurism—a disease previously always fatal—springs from the same root, and was discovered and verified in the same manner. Look at the operation of ovariotomy, by which one single surgeon has saved the lives of more than five hundred women. This process, or at least a certain point which it involves, was first proved upon dogs, rabbits, and guinea-pigs. Without such preliminary experimentation no medical man would have ventured on a procedure formerly held necessarily fatal. To declare, then, that vivisection, as a means of research, is barren and deceptive, betrays an amount of ignorance which is positively indecent. It is the bounden duty of all who undertake to enlighten the public on any subject to make themselves first accurately and fully acquainted with all its bearings. Otherwise, instead of enlightening, they deceive and lay themselves open to the natural suspicion that such is their intention. He who comes forward in a court of justice to give evidence on matters which he does not know is considered a perjurer. Is not the position of the humanitarian zealot who scatters baseless assertions broadcast morally, though not legally, exactly similar? Surely the few illustrations which we have cited, capable as they are of being indefinitely multiplied, form an overwhelming proof of the value of vivisection, and must utterly silence all the cavils raised against it on the score of inutility. Nay, they

ought, in our opinion, at least to be held decisive of the whole question. What, in opposition to such solid facts, are the hysterical utterances of Mænads who proclaim that their "hearts," and possibly other vital organs, are sick? Let no one say that we are advising to do evil that good may come; that which yields such essential services to the whole human race cannot be declared evil by any one, except he denies to man any rights over the brute creation whatsoever. The Royal Commissioners point out that most important researches, involving experimentation upon animal life, are now in progress. These relate to some of the "severest scourges which afflict the human race," such as cholera, consumption, blood-poisoning, typhoid, the bites of poisonous serpents. Are such investigations to be suspended? Were we prone to sensationalism, we might paint the shades of the thousands of our fellow-subjects who yearly perish in India from the bite of the thanatophidia hovering over the couch of the anti-vivisectionist fanatic, and shrieking in his ear that they might have been saved had he not begrudged the death of a few rabbits and guinea-pigs. No one can abhor more than do we the infliction of any wanton torment upon animals. We even object to all avoidable destruction of vegetable life. But if a remedy for the bite of the cobra alone could be found, we should consider it cheap if all the guinea-pigs in creation had been used up over the discovery.

There are those who declare that, rather than owe their life, or that of their children, to methods elaborated in such a manner, they are willing to die. It may be so, and we do not dispute their right to decide thus for themselves; but we entirely deny their right to speak for others. When they attempt this, when they seek to suppress vivisection, they fight—as humanitarians are very apt to do—not for liberty, but for supremacy. They seek to deprive others, who are not willing to die so long as a remedy can be found, of a source of benefit and safety.

But the boast of rejecting improved medical treatment founded upon the results of vivisection is idle and empty. They cannot, if they were to make the attempt. It is scarcely too much to say that whenever they consult a medical practitioner they enjoy more or less, and directly or indirectly, the benefits of discoveries made in this denounced manner. Can they efface from the mind of their physician or surgeon his knowledge of the circulation of the blood and all its consequences? Can they call up from their graves the ante- and anti-Harveians to treat their diseases? Surely, then, they are in a false position. They quench their thirst at a stream, and yet would seal up its springs!

We have now to ask, What is it that the anti-vivisectionists want, and what concessions would induce them to drop the subject and return to pursuits for which they are less glaringly unqualified? Here we must confess that, like all agitators, political or social, they enjoy a great and unfair advantage. With a Government, or a municipal body, or an incorporated company, it is at least possible to negociate, because there is in such cases some responsible head whose actions and words bind the body which he represents. But who is to bind an agitation like this? Suppose some of its most prominent representatives give to-day a formal statement of their views and of their requirements, to-morrow the whole transaction may be repudiated by a number of their coadjutors. The Society for the Prevention of Cruelty to Animals has, indeed, we suppose, an organisation and certain responsible officials, but can it bind all its members? Nay, have we not reason to suppose that every concession will be regarded as a mere instalment, to be immediately followed by fresh demands until total abolition is accomplished? This, and this only, and not regulation or Governmental inspection, is the real object held in view, and therefore we protest against the slightest concession. Before going in detail into some of the modest demands made upon the Legislature, we may consider what the term "Vivisection" is, in humanitarian circles, meant to cover. We shall be greatly mistaken if we confine the word to its strict grammatical sense. It is used where, accurately speaking, there is no "section" at all. Thus experiments upon animals to discover remedies for poisons or for diseases, or to test the action of some newly-discovered drug or chemical would be as decidedly inhibited as the cutting of a nerve or the irritation of some particular portion of the brain. Nay, it even appears that to place any animal under abnormal circumstances in order to observe the result would be criminal. For instance, the present writer has been engaged with experiments on the action of different colours of light, different temperatures and kinds of diet upon the development of certain insects. Should the anti-vivisectionists gain the day, he will have to discontinue his experiments under the prospect of imprisonment. Thus, in fact, physiological experimentation, as far as animals are concerned, will be at an end. Now let us look to the demands of the enemies of science, for as such they must be manifestly regarded. One proposal was "to render unlawful any experiment made for the mere (!) advancement of science." They could scarcely have given a more signal proof of their ignorance, and of their consequent unfitness

and want of right to interfere with the subject. Nearly three centuries have elapsed since Francis Bacon uttered the memorable truth that one scientific principle, rightly established, draws after it "whole squadrons of practical applications." During these three centuries his saying has been illustrated and verified, in every branch of science, times without end, and might, we hope, have penetrated even to the "persons of consideration" who are raising the present hubbub, and yet we find them babbling about the "*mere* advancement of science," as if too frivolous a matter for consideration! The Royal Commissioners gravely reject this absurd proposal. They remind its sapient originators that the onward steps which have immortalised Harvey and Galvani were, when first made, "mere scientific discoveries," and that "the germ of a great discovery is often so small as to be scarcely perceptible, and yet it may contain in it the grandest results." All this is, of course, true; but it is somewhat sad when the education of "persons of consideration" is so far in arrears that they require to be reminded of such elementary truths. Sadder still when persons so ignorant will come forward as teachers of the public, and seek to overbear the truth with sensational clamour.

Another proposal is not merely absurd, but disgusting. It was demanded that any person writing, publishing, or selling any book or journal containing an account of experiments made upon living animals, should, on complaint being laid, be summoned to appear before a police-magistrate or a court of petty sessions, and, failing to prove to the satisfaction of the Bench that the experiments described did not involve cruelty to animals, should be liable to fine and imprisonment, as well as to the forfeiture of the books. According to this suggestion, which is simply a standing disgrace to its authors, the English student would be debarred even from reading the results obtained by foreign authorities. The works of the most illustrious biologists and the Transactions of the most honoured Academies would be placed on a level with the literature of Wych Street. The shops of our medical publishers would be beset with "active and intelligent officers," whilst detectives and spies, amateur and professional, would "get themselves up" as medical students, and come asking for some volume of forbidden lore in order to entrap the unwary bookseller, while in the background lurked some Secretary prepared with every quirk and quibble the Law could furnish in support of the charge. Should this suggestion ever become Law, a long step will have been taken towards the re-introduction of the censorship.

Indeed it would be far less irksome for biological writers to submit their works to the preliminary judgment of a censor than to live in fear of being dragged up to prove their innocence of cruelty before an over-worked and not over-cultivated police-magistrate, a bench of Shallows—ignorant and careless of Science, or, in the worst case, before a jury of adulterating tradesmen. We are neither politicians nor lawyers, and the "Quarterly Journal of Science" is no political organ; still we feel free to point out that this proposed interference with the medical press, in throwing the burden of proof upon the accused, involves a departure from the ancient spirit of English law and an attack upon one of the safeguards of our personal freedom.

Another proposal, aiming at limitation rather than destruction, is, that only qualified medical men should be allowed to experiment upon animals. Now that biology is one of the sciences upon which the art of medicine reposes is quite true, just as astronomy is one of the bases of the art of navigation; but to restrict biological research to medical practitioners would be as absurd as to confine astronomical investigations to officers of the navy. Biology will be more and more cultivated by students not directly connected with the medical profession, and certainly not practising as physicians or surgeons.

Let us now examine what ground is taken by the Royal Commissioners who occupy an impartial position between Science and her enemies. Their concessions here will seem to the agitators too little, whilst to us they appear too great. As regards experiments for mere demonstration performed in medical schools* they consider such both necessary and permissible under the regulation which already exists,—that they be performed under the influence of some anæsthetic. It may be assumed that the good sense and feeling alike of professors and students will prevent such experiments from being needlessly multiplied. No one, for instance, in our day, would think it necessary to drop a mouse into a receiver of carbonic acid, or to exhaust it under the air-pump. Phenomena so superabundantly demonstrated may now, surely, be received on authority, without experimental evidence.

As regards research—by far the more important branch of the question—they accept the principles laid down by the Physiological Section of the British Association in 1871, to which they propose that legislative sanction should be given,

* In no others?

thus imposing what they designate as the "reasonable superintendence of constituted authority." All persons engaging in experimental research are to be licensed and registered by the Home Secretary, under certain conditions, and to be subjected to the visits of an Inspector. With this conclusion a medical contemporary expresses itself satisfied, considering that there is no reason to fear lest the proposed legislation should "in any material way" hinder the progress of research or diminish existing facilities, hoping that, on the other hand, such measures would "calm the needless apprehension, and put an end to the odious misrepresentations which have been recently rife concerning this subject, and which have been in ignorance adopted by persons of consideration, who will probably in future take more pains to be correctly informed."

In these anticipations we find ourselves unable to agree. We must remember that in England a large amount of scientific work has always been performed by persons holding no official position, graduates of no college, and—as Giordano Bruno says of himself—"academicians of no academy." These men are in the outset of their career necessarily unknown. Will they always be able to fulfil all the formalities necessary to obtain a license? The Home Secretary assuredly will not grant every application made as a mere matter of form, yet if he does not there will be a possibility that the progress of research may be checked in a "very material way." In such matters we should dread the interference of the British Government more than that of any other. An authoritative definition of vivisection, and a clear exposition of the conditions under which licences are granted, will or should be included in the Bill, and if these are satisfactory it will be perhaps the first time that the British legislature has dealt advantageously with a scientific subject.

The hope that such a regulatory Act will put an end to "odious misrepresentations," and induce "persons of consideration"* to seek correct information before entering upon a career of agitation, is amiable, perhaps child-like; but it will be doomed to disappointment. Since that Report was published the anti-vivisectionists have showed no symptoms of relaxing their endeavours, or of desisting from the dissemination of "odious misrepresentations."

* Can any person, of whatever rank, be considered "of consideration" when speaking upon a subject of which he is utterly ignorant, and on which he has not even sought for "correct information"?

On the contrary, a book has appeared in which the Royal Commissioners are accused of having suppressed important evidence! We have even heard it rumoured that attacks are being made through private channels upon men known or supposed to be engaged in biological experiments. If they are medical practitioners, their patients are to be cautioned against placing themselves in such dangerous and cruel hands. In whatever sphere of life they move they are to be cut off, as far as possible, from the good offices of society. It may be said that those who employ weapons thus obviously borrowed from the trades'-unionists lay themselves open to legal proceedings; but it is not very easy for a private man of perhaps limited resources thus to contend with the creatures of a wealthy combination. We cannot, indeed, lay our finger upon any of the agents who carry on this game, nor can we prove that this or the other "person of consideration" is cognisant of what is taking place. It is sometimes convenient to be able, with technical truth, to deny all knowledge of dishonourable actions. To whatever extent these attacks may be carried, we submit that the anti-vivisectionists have proved themselves to be *intransi gentes*. It is imprudent to make concessions to those whom no concession will satisfy.

There is still another reason why the Report of the Royal Commissioners should not be accepted. It is exceedingly bad policy to yield anything to agitators who have not made themselves acquainted with the facts of the case before disturbing the public peace and making unwarrantable and calumnious attacks upon private character. We think that all medical practitioners and all men of science are bound, in duty to themselves and the public, to oppose a firm and unbroken front to this senseless agitation.

We regret to find that Government has not merely decided to legislate on the subject of vivisection, but that the proposed measure will be more dangerous to science than we had any reason to fear. It is declared that—"A person shall not perform on a living animal any experiment calculated to give pain, except subject to the restrictions imposed by this Act." Here we come to the first difficulty. Who is to decide what experiments are or are not calculated to give pain,—a man of science or a humanitarian? If the latter, the wording of the clause might as well have been "any experiment whatsoever."

Again, not merely are the permitted experiments to be restricted to "registered places," and to be performed by "a person holding a license," but it is declared that "the

experiment must be performed *only* with a view to the advancement by new discovery of knowledge which will be useful for saving or prolonging human life or alleviating human suffering." Thus the point against which the Royal Commissioners wisely made a stand is conceded, and experiments for the advance of general biological knowledge—except bearing directly upon the treatment of disease—are evidently proscribed. Vivisection is to be simply medical in its object. That this most objectionable clause is to be strictly interpreted appears from a subsequent proviso:—"Experiments may be performed not directly for the advancement by new discovery of knowledge which will be useful for saving or prolonging human life or alleviating human suffering, but for the purpose of testing a particular former discovery alleged to have been made for the advancement of such knowledge as last aforesaid, on such certificate being given as is in this Act mentioned that such testing is absolutely necessary for the effectual advancement of such knowledge."

Hence it would appear that the licensed experimentalist is not at liberty to perform such operations as may to him seem calculated for the advancement of science, but that he may be called upon, in any individual case, to explain his exact object and mode of procedure, probably to an Inspector under the Act, who may use his own judgment as to whether the researches proposed are permissible. Experiments undertaken with a view to the prevention or cure of disease in domestic animals—surely an important object—will, as far as we can judge, be considered illegal. The scale of penalties proposed is exorbitantly high,—a point of the greater importance if we remember that experimentalists will be subjected to organised espionage, and that, if once they are accused of "cruelty," swearing of the stoutest quality will be employed to secure their conviction. We can only repeat our regret that one of the most important sciences should be placed in a position so humiliating.

We have heard of the "endowment of research" as a something vaguely looming in the distance. It is becoming tangible at last in the shape of penalties of fifty and one hundred pounds, supplemented with months of imprisonment! Instead of King Log we have now King Stork; active persecution instead of contemptuous neglect. There has been much said lately about a great scientific revival in England. Should this Bill and the Patent Law Amendment Bill pass into law the present administration will have given that revival a blow which no empty courtesies can heal, and

which will prove that whatever party is in power we have still "a Government very hostile to Science and to her disciples."* We may well exclaim, to Whig and Tory alike —"A plague on both your houses!"

IV. INFUSORIAL EARTH AND ITS USES.

By Dr. W. H. Wahl.

GEOLOGISTS have long since established, beyond peradventure, the fact that there are rocks in the interior of continents, at various depths in the earth, and at great heights above the sea, which are almost entirely made up of the remains of what were once living organisms. Such rock-masses, says Lyell, may be compared with modern oyster-beds and coral-reefs, and, like them, their rate of increase must have been extremely gradual. But there are a variety of mineral deposits that are now proved to have been derived from plants and animals, of which the organic origin was not suspected even by naturalists. Great surprise was therefore manifested when Prof. Ehrenberg, of Berlin, announced the discovery that a certain kind of siliceous stone, called tripoli, was entirely composed of the remains of countless millions of extremely minute organic beings. This observation of the famous German microscopist speedily led to the discovery of the fact that deposits of this character were quite abundant, and that they were even being formed at the present time over extended areas. The minute organisms, whose skeletons make up the bulk of the deposits which are now known under the name of infusorial earth, have been shown to inhabit the ocean in inconceivable numbers, giving rise to the luminosity of the waters, which has been the subject of much discussion, and flourishing in almost every place where water stands for several months of the year. Their indestructible shells are therefore to be found in greater or less quantity in the sedimentary deposits of all our bogs, ponds, and slow streams. They are found in great abundance beneath peat-bogs, where they constitute strata, often many feet in thickness and of great extent, almost entirely composed of the siliceous carapaces of organic beings, so inconceivably minute that millions of

* Eine der Wissenschaft und ihren Jüngern sehr abhold gesinnte Regierung.

their remains are found in a single inch. Ehrenberg estimates that about 18,000 cubic feet of these siliceous organisms accumulate annually in the harbour of Wismar, in the Baltic. He has furthermore demonstrated that they accumulate in the beds of American and other seas, lakes, and rivers.

The deep sea soundings which have lately been conducted in various quarters of the world, and have attracted much popular interest, have shown, likewise, that the impalpable mud or ooze which is accumulating at great depth in the bed of the Atlantic and other oceans is made up almost entirely of the mineral skeletons of certain extremely minute organisms. Of these shells, some are calcareous, and appear to be identical with the organisms which abound in the chalk of Europe—the chalk, indeed, is largely made up of such organic remains—while others are siliceous. One of these deposits in the North Atlantic has been traced over a distance of thirteen hundred miles in breadth, and not less than six hundred miles in length.

In peat-bogs, swamps, and the like, both of modern and ancient origin, there are often found layers, at times many feet in thickness, and of considerable extent, of a white siliceous paste, which is found beneath the microscope to be made up wholly of the remains of these minute organisms. These deposits, with which this article is chiefly concerned, are designated by geologists with the name of infusorial earth. The substance of which they are composed has generally, when dry, the appearance and consistence of friable chalk, and the remains of which it is made up, and which were formerly referred to microscopic infusoria, are now generally held to be plants, called by naturalists *diatomaceæ.* The remains of these diatomaceæ are of pure silex, and their shapes as seen beneath the microscope are various, and form objects often of extreme beauty. These forms are very marked and constant in particular genera and species, of which many hundreds have been described and classified by Ehrenberg, Bailey, and others, and while many of the fossil forms are identical with living species, others are allied to them; and the so-called infusorial beds are sometimes of marine and sometimes of fresh water origin. The infusorial earth may readily be distinguished from the several calcareous and clayey deposits which it resembles in appearance by the fact that it does not effervesce in acids, and its ready solubility in solution of caustic soda or potash. It has long been well known in the arts as a powder for polishing stones and metals. At Bilin, in Bohemia, which is

famous for its occurrence, there is a single stratum of this material, not less in some places than 18 feet in thickness, and extending over a large area. This stone, when seen beneath the microscope, is found to consist of the siliceous plates or frustules of the above-mentioned diatomaceæ, united together without visible cement, and so inconceivably minute are the particles of which it is composed that, according to Ehrenberg's statement, a single cubic inch, which weighs about 220 grains, contains about 41,000,000,000 of individuals, and a single grain no less than 187,000,000. Other deposits of infusorial earth (kieselguhr), scarcely less extensive, occur in Germany, at Berlin, and at Planitz, in Saxony. It is found near Lüneburg, in a stratum nearly 28 feet in thickness, and again at Kliecken, near Dessau, and in the vicinity of Cassel. In England, deposits of considerable magnitude have been found in Surrey, at the base of the chalk hills, and elsewhere. In Ireland there is a celebrated stratum on the banks of the River Bann, in the county of Down, and which, from being in much request for polishing plate, is locally known as Lord Roden's plate powder; another bed is found at the base of the Mourne Mountains in the same county. In Lapland a similar earth is met with, which in times of scarcity, it is said, is mixed by the inhabitants with the ground bark of trees and used for food. The edible earth of Lillhaggsjon, in Sweden, is of the same nature. The infusorial earth is found in quantity in the Isle of France and at San Fiora, in Tuscany, and deposits of various thicknesses have been detected in Africa (the tripoli of commerce is an infusorial earth that has long been exported from the country whose name it bears), Asia, Australia, and New Zealand. In America it has been found in a great number of localities, and occasionally in enormous quantities. Of this nature are the beds of white earth along the banks of the Amazon, in Brazil, and used occasionally as food (?) by the native inhabitants. They have been detected also in Newfoundland and Labrador. In the United States perhaps the most remarkable deposit is that upon which the City of Richmond, Va., is built; this deposit is, in places, over 20 feet in thickness, and has been traced by Prof. W. B. Rogers, who was the first to point out its nature, from Herring's Bay on the Chesapeake, Md., to Petersburg, Va., and beyond. At Petersburg the stratum is 30 feet in thickness. Beds of the same character and of some magnitude have likewise been found in California, Oregon, and other points on the Pacific, and at West Point, N.Y.; while of less importance are the infusorial beds at

Wrentham and Andover, Mass., Smithfield, R.I., Stratford, Conn., and other localities too numerous to mention.

An interesting occurrence of this nature is the deposit of infusorial earth at Drakeville, Morris County, New Jersey, and which, through the instrumentality of the writer and others, was first brought into general public notice about three years ago. The bed in question is on the property of the late Frederick S. Cook, and is located at the foot of Schooley's Mountain. The annual report of Prof. George H. Cook, State Geologist of New Jersey for 1874, contained a descriptive article in reference thereto, from which we obtain the following statements concerning its probable extent, &c.

"It has been known as a white earth or marl for a long time, and some years since was dug out and spread upon the soil as a manure; it had also been observed to possess remarkable excellence for scouring silver. The establishment of a manufactory for making nitro-glycerine and giant powder at McCainsville, near Drakeville, in which infusorial earth imported from Germany was used, led to an examination of this deposit, when it was found to be the same material with that they were bringing from Europe. The deposit occurs in a depression of the surface just at the foot of the mountain (Schooley's). The swale appears to be occupied in its lowest part by a common swamp of low bushes, growing in wet black earth; but by digging in the black earth it is found to be only about a foot thick, and underneath it is the infusorial earth. The extent of the black ground is about 540 feet in length by 200 feet in breadth, and 100 yards north-east is another but much smaller deposit. A trial pit sunk in the middle of the swale showed a thickness of 12 inches of black earth, 8 inches of very light infusorial earth, and 12 inches or more of a much denser infusorial earth. The lower part is said to be 3 feet thick, but I only examined the upper foot of it."

The report continues:—"There is little doubt that other deposits will be found in the small ponds and swamps in this gneiss region, and those interested will do well to make search for it in any of the swales where these little swamps occur. It can be easily reached by digging, and when found can be distinguished from any other white earth by its not effervescing with acids as white marl does, by its not becoming plastic when wet, as white clay does, and by its dissolving almost entirely in a strong boiling hot solution of washing soda.

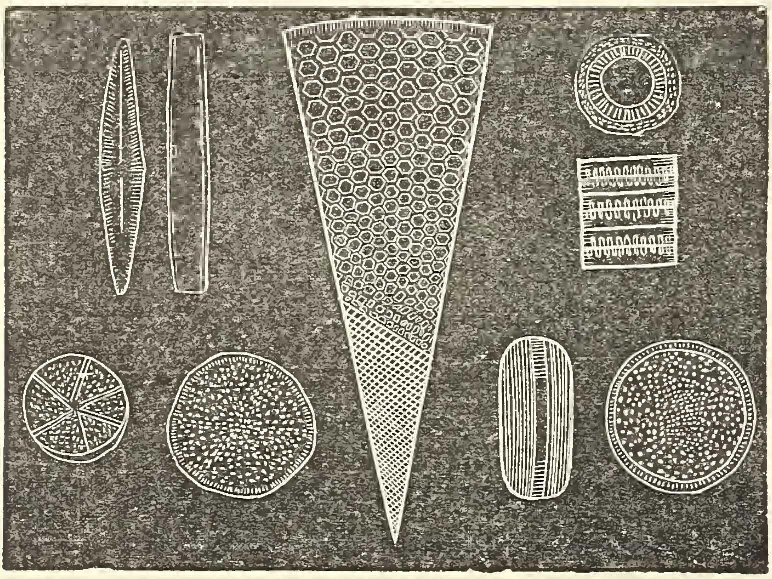
"The importance of this material will be appreciated when it is stated that the manufacture of *dynamite*, or giant

powder, at Drakeville, has reached £50,000 a month. There are different grades of *dynamite*, but some of it contains 25 per cent of infusorial earth."

An analysis of an average dried sample of the Drakeville deposit yielded the writer 47·12 per cent of soluble silica.

Concerning the application of this curious substance in the useful arts quite a chapter might be written. During the past few years it has attracted the special attention of practical men, and so many and various have been the uses for which it has been suggested that their bare enumeration may well excite surprise. At least one very important industry of recent origin has been practically created by it, and its employment in others is steadily growing in extent and importance. A summary of the subject in its technical aspects, with brief comments upon the more important items, is given in what follows.

The most popularly known and perhaps the earliest application of the diatomaceous earth is its utilisation as a



polishing agent for stone and metals. For this purpose, when carefully freed from grit and other impurities, its considerable hardness and its wonderfully fine state of division fit it most admirably. It may be applied wet or dry. It is well known in this connection under the name of tripoli, so called from the locality whence it was originally brought. Under the name of "electro-silicon," "magic-brilliant," and other trade designations, the diatomaceous earth from Nevada and other localities has been extensively introduced as a polish for gold, silver, and plated ware, for which—as for tin, Britannia ware, and other metals used in the household—its wide popularity is the best proof of its excellence.

Being a very poor conductor of heat, it has been suggested and applied for surrounding ice, beer and ale cellars, fire-proof safes, steam-boilers, powder-magazines, refrigerators, &c. The results of certain experiments lately made by Refardt and Co., of Braunschweig, to ascertain how this material compared with other substances generally employed for the purpose, are highly favourable to the merits of infusorial earth for this application.

Without entering into the mechanical details of the apparatus employed in these trials, it will suffice to state that in the time required to melt 100 parts (by weight) of ice surrounded by the siliceous earth, 235 parts of ice were melted in a cylinder surrounded with an equally thick layer of dry, light garden earth. Moist earth, and moist materials generally, gave still more unfavourable results. Again, for every 100 parts of ice melted when protected by the infusorial earth, 142 parts of ice protected by dry, sifted coal ashes, were melted. The results obtained with flax-shives were about the same as with the infusorial earth. These trials demonstrated that infusorial silica and flax-shives offer the greatest amount of resistance to the transmission of heat; that dry coal-ashes are far less efficient, and moist ashes still more so; and finally that earth, as compared with these, is very inferior as a non-conductor. The use of the infusorial earth is therefore highly recommended for filling in between the walls, and for covering the mason work in ice-cellars. For this purpose the following additional advantages are urged in favour of this substance, viz.:—It is extremely light,—being nearly five times as light as dry earth, and about half the weight of dry coal ashes,—and it is not combustible, remaining unaffected in the hottest fire. These properties, to quote from the published account of the above trials, render this substance preferable to flax-shives, tan-bark, peat, saw-dust, and similar materials, which are about equal to it in non-conducting quality, but which are combustible, and when kept for some time rot or moulder, shrink and settle, and might under some circumstances, take fire spontaneously (*sic!*).

The infusorial earth, it is further claimed, will be found highly useful in fire-proof safes, as a surrounding for powder-magazines on shipboard, for covering steam-pipes and boilers, and for all similar purposes. Reference is made in some of the encyclopædias (*vide* "American Encyclopædia," iii., 268) to what are termed floating bricks, which, according to account, are made of infusorial earth, and are named in virtue of their power of floating upon water. Clay is some-

times added to the silica to assist in binding the material together. Such bricks, we are told, were made in ancient times, and were described by Posidonius and Strabo, and particularly commended by Vitruvius, Pollio, and Pliny. In 1791 they were again brought into notice by Giovanni Fabroni, in Tuscany, who, after many trials, succeeded in making bricks which would float upon water. Their strength was but little inferior to that of ordinary bricks; they are remarkable not only for extreme lightness, but also for their infusibility, and for being very poor conductors of heat; they may be held at one end while the other is red-hot. As an experiment, Fabroni constructed the powder-magazine of a wooden ship with these bricks; the vessel being set on fire, sank without explosion of the powder. In 1832 Count de Nantes, and Fournet, a mining engineer, used them in constructing powder-magazines and other parts of ships, thus lessening danger from fire. From an earlier source ("Encyclopædia Americana," ii., 266) we are informed that these floating bricks, made of agaric mineral or fossil farina, —infusorial earth,—has been found, on account of its infusibility at the highest temperatures, to be extremely useful in constructing reverberatory furnaces, pyrometers, and magazines of combustible materials, while their lightness and non-conducting qualities render them particularly useful for the construction of powder-magazines on board of ships.

In agriculture, the use of the infusorial earth has been suggested as a manure for lands poor in silica, which substance enters importantly into the constitution of the stalks and outer coverings of cereals. Quite an animated controversy, indeed, has of late sprung up as to the merits of infusorial silica as a component of fertilisers, an idea which forms the essential feature of a patent lately issued to Messrs. N. and G. Popplein, Jun., of Baltimore. It would be foreign to the purpose of this sketch to enter into a discussion of the merits of this controversy, involving as it does the introduction of certain debatable questions in agricultural chemistry; but the ideas of the Messrs. Popplein have aroused on the one hand such warm championship, and on the other such opposition, that a concise statement of the points in dispute may not be amiss.

The manufacturers before named, proceeding from the well-known fact that the relative quantity of silica in the ash of the cereals is greatly in excess of what is required for the normal combination with the bases (potash, soda, &c.) found therein, claim that in the ordinary course of things it is impossible for Nature to furnish to cultivated lands for

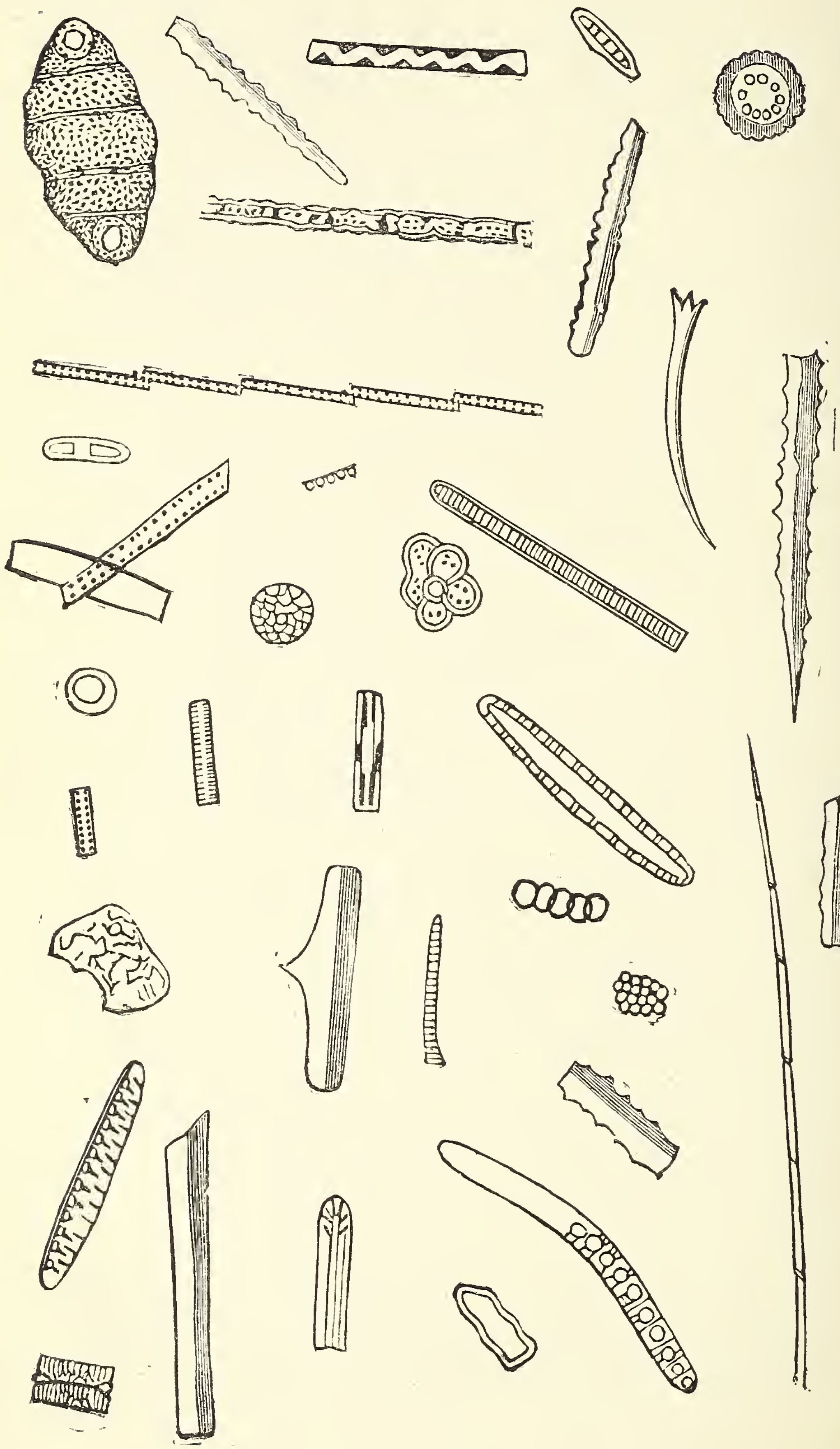
successive years the proper amount of silica in assimilable form for the plant, inasmuch as the liberation of this substance by the chemical decomposition of the mineral matters of the soil containing it, goes on so slowly as to render doubtful the production in many years of the amount required for a single crop. In proof of this assertion they refer to the great reduction in the yield of the wheat crop, since farmers began years ago to sell the straw of the crop that formerly was returned to the soil. For this, and other reasons less obvious, their attention was attracted to the importance of incorporating silica into commercial fertilisers —one difficulty remained to be overcome, namely, the discovery of a form of the silica—which should be assimilable by the plant. This they claim to have found in the infusorial earth,—in which the silica is in an inconceivably minute state of division,—the result of their consideration being the production of a so-called "silicated superphosphate of lime," a superphosphate with which the infusorial earth is intimately incorporated. The argument urging the importance of an abundant supply of silicic acid in available form, as an absolute necessity for the proper nutrition of cereals, is not disputed; and the manufacturers, to demonstrate the availability of the silica in the form in which they employ it, have actually succeeded in proving beyond question the highly interesting and novel fact that the very minute skeletons or shells of which the infusorial earth is mainly composed are carried up *as such* into the body of the plant itself. Upon this point the following gleanings from an investigation conducted by Prof. P. B. Wilson will be read with interest:—

This chemist subjected to a microscopical examination the straw from the wheat-fields of Col. J. B. Kunkel, of Frederick County, Maryland, which had been fertilised by the silicated phosphate, his purpose being to make "a more complete investigation into the siliceous structure of the stalk, in determining whether the Infusoria passed directly as such into the sap-cells, to be carried forward by capillary force, and to finally assume their functions,—the formation of the epidermal shield for giving strength to the straw, to withstand the destructive force of high winds and beating rains, as well as a protection against the attacks of parasites."

"In making these investigations thorough precautions were observed, to cleanse the straw from all accidental impurities by washing and gentle friction, not sufficient, however, to destroy the epidermis. The organic matter was

Forms of Diatoms found in Col. Kunkel's Straw.

MAGNIFIED 300 DIAMETERS.



then removed by the prescribed methods, aided by my own experience."

"My labours," he continues, "have been amply rewarded by one of the most enchanting views that has ever fallen to my lot to behold through twenty years of varied scientific investigations. When the epidermal siliceous coating was adjusted upon the field of the microscope, some thirty-six forms of the Diatomaceæ, which I have carefully sketched, were observed (see engraving, magnified 300 diameters) where perfect disintegration has been produced. When the structure to a great extent is retained a marvellous interlacing of these forms presents itself, sometimes side by side, at other times overlapping."

From this very interesting observation, Prof. Wilson advances a number of inferences, which, although the writer is not prepared to accept in full, are of sufficient interest to warrant their reproduction. He affirms that his investigation "overthrows all theories that have ever been advanced, that silica enters into plant structure in combination with the alkalies, the alkaline earths, or the earths proper. Chemical investigation led me to this conclusion some months since, now confirmed by that of the microscope."

"My mind was particularly impressed with the absence of the disc-like form, the *Actinocyclus ehrenbergii* and the *Actinoptychus undulatus* in their perfect state in the straw, while the other forms are common both to the infusorial earth and the wheat. My conclusions are that the varieties mentioned are too large to enter the root capillaries, for on the field of the microscope they have three to four times the magnitude of the others. This I will fully investigate during the coming summer, by making accurate measurements of rootlets and diatoms, when I will be able to obtain stalks of wheat as grown in the fields, preferring this mode of investigation to *pot culture*, to disarm controversy, and to divest the investigation of all semblance of laboratory experiment."

"I have examined various specimens of wheat straw taken at random from the market, but have failed to find a single diatom. This to a certain extent surprised me, when taking into consideration that they are found to a limited extent in Peruvian guano. The inference to be drawn is, that the soil was not fertilised by any material into which it entered as a constituent. I mention this to guide others who may make subsequent investigations from falling into error, in case occasional Diatomaceæ are observed, as being derived from other sources than the infusorial deposits."

"These microscopic investigations show the absence of

other forms of silica, that is, in granular particles in the (Kunkel) straw, they being entirely replaced by diatoms. This leads to the conclusion that the diatom is the more acceptable for assimilation, and when sufficient infusorial remains are present, replaces any other divided form of silica. I have previously attempted to substitute silica for diatoms, as obtained from the decomposition of slags from iron furnaces, but have failed to derive any satisfactory results. This is due to its combination as a silicate; and when liberated by stronger acids, it agglutinates into masses too hard and large to be absorbed by the plant."

Prof. Wilson concludes his report in the following glowing terms:—" I look upon this application of vegetable silica to fertilising purposes as the most important adaptation of matter for the reproduction of vegetation that has ever been discovered. It is the first step in a new direction, rationally conceived and judiciously carried out. A new impetus will be given to the study of plant physiology, which will demonstrate that more than a heterogeneous mixture of elementary bodies and their compounds are required for the production of the crops beneficial to the requirements of man."

With regard to the foregoing statements and inferences of Prof. Wilson, while not attempting to undervalue their great interest and possible entire accuracy, the writer would remark that the demonstration of the presence of the infusorial forms in the structure of the wheat stalks proves simply that these bodies are sufficiently minute to enter the root capillaries and pass into the sap-cells of the plant—nothing more. It may possibly be that, once having entered the body of the plant, they are assimilated, and made to subserve to the function of giving strength to the stalk; or, as appears to the writer equally plausible, they may simply act as so many minute mechanical impurities drawn into the circulation of the plant, and, effecting a lodgment wherever they chance, clog up the passages, and thus actually obstruct rather than serve the process of nutrition. To follow the history of one of these forms in a living plant under the microscope, and observe its gradual dissolution, would afford the only method of positively proving the truth or falsity of either of the explanations that have been presented. While not presuming to decide so doubtful a question, it is very reasonable to believe that much of the silica of the so-called silicated superphosphate is made "available" as plant-food in solution as an alkaline salt, in which condition its assimilation by the plant presents no difficulties to the understanding. Dr. Wolf, the excellent

State Chemist of Delaware, has kindly furnished the writer the following record of an analysis of the "silicated superphosphate," viz.:—

Soluble Phosphoric Acid . .	5·855	per cent
Precipitated Phosphoric Acid .	3·327	„
Insoluble Phosphoric Acid .	trace	
Silica	20·568	„
Sulphate of Potassa	6·173	„

According to the Messrs. Popplein's published formula, the net ton of their "silicated superphosphate" contains—

Infusorial Earth	800 lbs.
Dissolved Bone	800 „
Potash Salts	400 „

As an absorbent and carrier of liquids of various kinds, and especially as a carrier of nitro-glycerine, the infusorial earth has been found to be most excellently adapted. It takes up from three to five times its weight of water, oil, nitro-glycerine, &c. It would doubtless prove equally valuable as a carrier of carbolic acid and other disinfectants, as a disinfecting powder, and has possibly already found application for this purpose.

In order to bring nitro-glycerine within the range of articles of transport, Nobel, who first demonstrated its value in the arts, devised the production of the powder now so extensively employed under the name of dynamite, in which the explosive oil is simply carried by the inert, pulverulent siliceous earth. The process of the preparation of dynamite may be described as follows:—

The infusorial earth must first be freed from water, organic substances, and mechanical impurities (sand, &c.) The first two are removed by calcining at a red heat in an oven with several shelves, one above the other, on which the earth is placed and slowly pushed from the upper to the lower. The organic matter, which is considered dangerous to the stability of the dynamite, is thus burned out. It is then pressed with hard rollers and sifted, which separates it from the larger particles and grit. It is now ready for use.

Fifty pounds of infusorial earth are put into flat wooden tanks and covered with 150 pounds of nitro-glycerine, when the workmen mix them with the naked hand. Gloves of india-rubber were at first provided, but the workmen preferred to knead the mixture with the free hands. In half an hour the incorporation of the oil with the earth is complete, and the dynamite is ready for filling in the cartridge

moulds. The cartridges are simple cylinders, protected by parchment paper. If ordinary paper is used the oil soaks into it, and there is great danger of premature explosion. Dynamite is a brownish grey, sometimes reddish, inodorous, pasty, greasy mass, having the specific gravity of 1·6. When ignited by an ordinary flame, it burns up quickly without detonation, and must therefore be fired with a patent exploder containing fulminate of silver inclosed in a copper capsule. It requires a heavy blow of a hammer on an anvil to explode it, and even then only the portions struck are fired. In this respect it presents great advantages over nitro-glycerine, which is easily exploded by percussion. On the other hand, the wood of the boxes in which dynamite is packed becomes by slow degrees impregnated with nitro-glycerine, and forms a most dangerously explosive material, which may give rise to serious accidents in warehouses where it is stored. As long as the nitro-glycerine is confined in the infusorial silica there appears to be very little danger, but the escape of a few drops of the oil may be the source of great mischief. The force exerted by the dynamite is much greater than that of gunpowder, and under the name of giant powder it has been largely employed in the mines of California. Other explosives, such as dualine and lithofracteur, may be said to be varieties of dynamite, having nitro-glycerine for their base, and using saw-dust or some other substance as an absorbent. All of them are powerful explosives, and must be handled with care.

For the preparation of cements and of artificial stone a number of processes have been devised, in which infusorial earth plays a prominent part, viz.: Equal parts of infusorial silica and litharge, and one-half part of slaked lime, stirred to a paste in linseed oil, is affirmed to become as hard as sandstone on setting, and is recommended as an excellent compound for cementing stone, metal, and wood. The following recipe, again, is pronounced to be serviceable for the production of an artificial stone for art objects. For this purpose the infusorial earth is intimately mixed with well-pulverised, freshly-burned lime, in the proportion of from three to six parts of the former to one of the latter. The mixture is then pressed into moulds under an addition of a very slight quantity of water. The resulting product, a silicate of lime, is formed with the evolution of considerable heat. The objects produced ultimately attain great hardness; they are perfectly water-proof, and may readily be coloured with any colour used in stereochromy.

In combination with sulphur, infusorial earth forms a

plastic mass, called *zeiodelite*; but no uses have yet been made of it.

By far the most important application of infusorial earth in this direction, however, has been successfully accomplished by Mr. Frederick Ransome, of England, in the production on the large scale of an artificial stone for general purposes, to which he has given the name of apoenite. The so-called "Ransome stone," invented by this gentleman, is made by thoroughly incorporating sand and silicate of soda in a mixing mill, moulding into the form required of the block, and then saturating the same with a solution of chloride of calcium, either by exhausting the air with air-pumps, or by forcing the solution through the moulded mass by gravitation or otherwise. The result is the formation of an insoluble silicate of lime, which firmly cements the particles of which the mass is composed, and of chloride of sodium or common salt, which is subsequently removed by the free application of water. The process of washing to remove all traces of the salt from the Ransome stone, which is necessary to prevent its efflorescence and secure its proper cementation, was found to be in many cases so tedious, expensive, and objectionable that the inventor, after many experiments, devised the following process, in which the use of chloride of calcium is avoided. Mr. Ransome mixes suitable quantities of lime (or substances containing lime) and soluble silica (*i.e.*, infusorial earth) with sand, and a solution of silicate of soda or potassa, which, when intimately incorporated, are moulded as before, and allowed to harden gradually, as the silicate of lime, produced by the action of the lime on the silicate of soda, is formed. As rapidly as the soda (or potash) of the water-glass solution is set free, it dissolves some of the infusorial silica, and again gives it up to the lime to form more cement, acting thus as the carrier of silica to the lime, until eventually all the lime is combined. In the course of the successive changes that take place, a portion of the free alkali appears to be bound at each step, with the lime as a compound silicate; and as the result of these several changes the whole of the alkali is gradually fixed, thus leaving nothing to be washed out. The mass gradually becomes thoroughly indurated, and in a very short time is converted into a very compact stone—apoenite—capable of withstanding enormous pressure, and increasing in strength and hardness with age.

In combination with magnesite (carbonate of magnesia), infusorial earth forms what is described as an excellent

cement, which is manufactured in Germany, and sold under the name of "albolite."

In pottery the infusorial earth has received several important applications. When fused, for example, with borate of lime, as such is obtainable in the trade under the name of boronatrocalcite or tincalzite, an excellent glazing is produced ("Manufacturer and Builder"), which is not only useful for furnaces and pottery of all kinds, but also for enamelling iron and slate, being free from lead and not apt to crack off. By fusing a mixture of infusorial earth (freed from sand) with borate of magnesia (stassfurtite), a kind of "hot-cast porcelain" is produced, having great durability and beauty. For this purpose the infusorial earth requires to be perfectly dry and free from lumps. It is introduced into the crucible in small portions and under constant stirring, until the fused stassfurtite ceases to take up more. The mass may be cast like glass, and if very liquid it may even be blown, and is thus fitted for an extensive application (*ibid.*).

Bœttger publishes the observation that when an alcoholic solution of any of the coal-tar colours is mixed with a sufficient quantity of infusorial earth, water added, and the mixture filtered, the liquid will run off clear, while the earth retains all the pigment. Hitherto the compounds of alumina have been used for the production of the so-called lakes, and it is quite probable that the above-noted behaviour of this material may find important applications in the arts.

The use of infusorial earth has been suggested in glass-making as a substitute for sand; but it appears not to be well suited for this purpose, the reason assigned being that it swells too much in the crucible. In the manufacture of soluble glass (water-glass), for which it has likewise been tried, the impurities it contains—clay, phosphate of lime, &c.—have been found to render it somewhat unsuitable.

To conclude a sketch which has unwittingly taken considerable proportions, the following enumeration will suffice to show that the subject is by no means exhausted:—A compound called diatite, devised by Merrick, consists of gum-lac and infusorial earth. The siliceous earth has been added to sealing-wax to prevent its running; it is sometimes added to paper to give it body; and to soap for the same purpose, and to add to its detergent qualities (?); and it is said to form an excellent addition to rubber, for certain uses of the latter; its addition to modelling clay is said to prevent it from cracking in moulding; and lastly,

though doubtless many real or suggested applications of this curious substance have been overlooked, it is said to be of use in the manufacture of smalt and ultramarine.

V. THE NIZAM DIAMOND—THE DIAMOND IN INDIA.

By Captain RICHARD F. BURTON.

IT is impossible to quit Golconda without a word concerning the precious stone which, in the seventeenth century, made its name a household word throughout Europe; and also without noticing the great diamond whose unauspicious name, Bala (little) Koh-i-núr, I would alter to "The Nizam." It is a singular fact that professional books,—like Mr. Lewis Dieulafait's "Diamonds and Precious Stones" (London: Blackie, 1874),—which give full particulars of all the historic stones, have utterly ignored one of the most remarkable.

The history of the Nizam diamond is simple enough:—About half a century ago it was accidentally found by a Hindu Sonár (goldsmith) at Narkola, a village about 20 miles east of Shamsábád, the latter lying some 14 miles south-west of the Lion City, Haydarábád, on the road to Maktal. It had been buried in an earthen pipkin (Kotí or Abkhorah), which suggests that it may have been stolen and was being carried for sale to Mysore or Coorg. The finder placed it upon a stone, and struck it with another upon the apex of the pyramid. This violence broke it into three pieces, of which the largest represents about half. With the glass model in hand it is easy to restore the original octohedron. The discovery came to the ears of the celebrated Diwan (minister) Rajah Chandù Lál, a friend of General Fraser, who governed the country as Premier for the term of forty-two years. He very properly took it from the Sonár before it underwent further ill-treatment, and deposited it amongst his master's crown jewels.

The stone is said to be of the finest water. An outline of the model gives a maximum length of 1 inch 10·25 lines, and 1 inch 2 lines for the greatest breadth, with conformable thickness throughout. The face is slightly convex, and the cleavage plane produced by the fracture is nearly flat, with

a curious slope or groove beginning at the apex. The general appearance is an imperfect oval, with only one projection which will require the saw: it will easily cut into a splendid brilliant, larger and more valuable than the present Koh-i-núr.

I can hardly wonder at this stone being ignored in England and in India, when little is known about it a Haydarábád. No one could tell me its weight in grains or carats. The highest authority in the land vaguely said "about 2 ounces or 300 carats."* The following statement has been made by Mr. Briggs:—"Almost all the finest jewels in India have been gradually collected at Haydarábád, and have fallen into the Nizam's possession, and are considered State property. *One uncut diamond alone of* 375 *carats is valued at* 30 *lakhs of rupees*, and has been mortgaged for half that money."

For uncut stones we square the weight (375 × 375 = 140,625) and multiply the product by £2, which gives a sum of £281,250. For cut stones the process is the same, only the multiplier is raised from £2 to £8. Thus, supposing a loss of 75 carats, which would reduce 375 to 300 (300 × 300 = 90,000); on multiplying the product by £8 we obtain a total value for the Nizam's diamond of £720,000.

I will now briefly compare the Nizam diamond—uncut 375 carats, cut 300—with the historic stones of the world. The list usually begins with the Pitt or Regent, the first cut in Europe. When the extraneous matter was removed, in unusual quantities, it was reduced to 136¾ carats, valued from £141,058 to £160,000. The Koh-i-núr originally gauged 900 carats; it was successively reduced to 279 or 280 (Tavernier), and to 186¼ (=£276,768) when exhibited in Hyde Park; its latest treatment left it at 162½ carats. Then we have the Grand Duke's, or Austrian, of 139½ carats (=£153,682); the Orloff, or Russian (rose cut), of 195 (193?) carats; and the Abaïte, poetically called the Star of the South, weighing 120 carats. The "Stone of the Great

* Our diamond weights are as follows:—

16 parts = 1 (diamond) grain = 4-5ths grain troy.
4 diamond grains = 1 carat = 3-1-6 (3·174 grains troy).

The Indian weights are—

1 dhan = 15-32 grains troy; in round numbers ½ a grain.
4 dhary = 1 rati = 1⅔ grains troy.
8 rati = 1 masha = 18 grains troy.
12 mashas = 1 tola = 180 grains troy.

The "ounces" in the text probably represent "tolas," certainly not troy ounces of 24 grains.

Mogul," mentioned by Tavernier, is probably that now called Daryà-i-núr: it weighs 279 9-16 carats, and graces the treasury of the Shah. The nearest approach to "The Nizam" is the Mattan or Laudah diamond, of 376 carats. Experts agree to ignore the Maganza, whose 1680 carats are calculated to be worth £5,644,800: the stone is kept with so much mystery that it is suspected to be a white topaz.

Diamonds have been found in the Ganges Valley. They are still washed as far north as Sambalpúr and the Majnodi, an influent of the Mahanadi; on the Upper Narbada (Nerbudda), on the line of the Godaveri and on the course of the Krishna. The extreme points would range between Masulipatam and the Ganges Valley: the more limited area gives a depth from north to south of some 5° (=300 direct geographical miles), beginning north from the Central Provinces and south from the Western Ghats, a breadth averaging about the same extent, and a superficies of 90,000 miles. A considerable part of this vast space is almost unexplored.

The history of the diamond in India begins with the Mahabharata (B.C. 2100). The Koh-i-núr is supposed to have belonged to King Vikramaditya (B.C. 56) and to a succession of Moslem Princes (A.D. 1306) till it fell into the hands of the Christians. At what period India invented the cutting of the stone we are as yet unable to find out; the more civilised Greeks and Romans ignored, it is suspected, the steel wheel. The Indian diamond was first made famous in Europe by the French jeweller, Jean Baptiste Tavernier (born 1605, died 1689), who made six journeys to the Peninsula as a purchaser of what he calls the Iri (hira).

Tavernier's travels are especially interesting to diamond-diggers. He began with "Raulconda," in the Carnatic, some five days' journey south of Golconda, and eight or nine marches from Vizapore (*hodie* Bijapur). In 1665 the diggings were some two hundred years old, and they still employed 60,000 hands. The traveller's description of the sandy earth, full of rocks, and "covered with coppice wood, nearly similar to the environs of Fontainbleau," is applicable to the Nizam's country about Haydarábád. The diamond veins ranged from half an inch to an inch in thickness, and the gangue was hooked out with iron rods. Some of the stones were valued at 2000, and some even at 16,000 crowns; the steel wheel was used for cutting. Tavernier then passed on to the Ganee diggings, which the Persians call Coulour (*hod.* Burkalún), also belonging to the King

of Golconda. These diggings lay upon the river separating the capital from Bijapur. The discovery began about A.D. 1565 with a peasant finding a stone gauging 25 carats. Here, we are told, appeared the Koh-i-núr (900 carats), which "Mirzimolas," or "Mirgimola," the "Captain of the Moguls," presented to the Emperor Auranzib. The 60,000 hands used to dig to the depth of 10, 12, or 14 feet, but as soon as they met with water there was no further hope of success. Tavernier's last visit was to "Soumelpore" (Sambalpur) "a town of Bengala, on the River Gowel," a northern affluent of the Mahanadi. The season for washing the diamantiferous land began in early February, when the water ran clear,—other authors make it extend from November to the rainy season,—and the 8000 hands extended their operations to 50 *kos* up stream. Gold, and the finest diamonds in India—locally called "brahmans"—were found in the river bed and at the mouth of the various feeders.

In 1688 and 1728 Captain Hamilton describes the diamond mines (probably those of Partial in the Northern Circars) as being distant a week's journey from Fort St. George, and records the fact that the Pitt diamond was there brought to light.

The diamond was practically limited to Hindostan and Borneo before A.D. 1728, when diggings were opened in the Brazils. The specific gravity of the diamond averages 3·6, and the difference of oxide in the crystallised or allotropic carbon does not exceed the third place of decimals. This, however, makes all the difference in lustre, for a small brilliant of perfect water is far more effective as an ornament than a larger stone of inferior quality. As far back as 1868 my study of the Brazilian diamond formation enabled me to prognosticate that the gem would be found in places where its existence had never been suspected. Since that time diamonds have been found in the Cudgegong River, near Rylston, New South Wales, and more recently in South Africa: these stones are inferior to those of the Brazils, yet they have reduced the value of the latter by one-third.

"The diamond mines of Golconda," according to Mr. Briggs, "derive their name from being in the kingdom of Golconda, and not from being near the fort. They are at the village of Purteeali (Partiál), near Condapilly, about 150 miles from Haydarábád, on the road to Masulipatam.*

* Mr. Maclean kindly drew my attention to the Treaty with the Nizam (Nov. 12, 1766), which cedes to the East India Company "the five Circars or provinces of Ellour (Ellore, north of Masulipatam), Rajahmoudra Siccacole

The late Nizam retained possession of them when he ceded the Northern Circars to the English Government. They are superficial excavations not more than 10 or 12 feet deep in any part. For some years past the working of them has been discontinued, and there is no tradition of their having ever produced very valuable stones."

Mr. Briggs's report is full of errors. He must have known that the Pitt diamond—one of the finest and most perfect of its kind—was produced at Gáni Partiál, and that the Koh-i-núr came from the so-called "Golconda mines." Again, Partiál — on the north bank of the Krishna, some 50 miles from the Bay of Bengal—is only one of many diggings in the vast area which I have before indicated, some being still worked, and the others prematurely abandoned.

The student will do well to consult the "Geological Papers on Western India" (Bombay, 1857), edited by my old friend Dr. Henry J. Carter. Here he will find detailed notices of a number of mines. John Malcolmson, F.R.S., treats of the diggings at "Chinon on the Pennar" and the Cuddapah mines. Of the latter Capt. Newbold says—"The diamond is found in the gravel beds of the Cuddapah district below the Regur"—the black, tenacious, and fertile soils of Central and Southern India. The same scientific officer also describes the yield of Mullavelly (or Malavilly) northwest of Ellora, as occurring in a bed of gravel, composed chiefly of rolled pebbles of quartz, sandstone, chert, ferruginous jasper, conglomerate, sandstone and kankar, lying in a stratum of dark mould about a foot thick. Both these geologists inferred the identity of the sandstone of Central with that of Southern India from the existence of the diamond at Weiragad, a town about 80 miles south-east of the capital. Malcolmson declared that the "celebrated diamond mines of Partel (Partiál), Banaganpilly, and Panna, occurring in the great sandstone formations of Northern India, as well as the limestones and schists associated with them, exhibit the same characters from the latitude of Madras to the banks of the Ganges, and are broken up or

(or Chicacole on the Coast), and Moortizanuggur or Gunton. The first four named were added to the French dominions by De Bussy. "These Circars," we read, "include territory extending along the coast from the mouths of the Kistna (Krishna) northward to near Ganjour, and stretching some distance inland." Article No. 11 of the same treaty runs thus:—"The Hon'ble E. I. Company, in consideration of the diamond mines, with the villages appertaining thereto, having been always dependent on H. H. the Nizam's Government, do hereby agree that the same shall remain in possession now also."

elevated by granite on trap rocks, in no respect differing in mineralogical characters or in geological relations."

The Rev. Messrs. S. Hislop and R. Hunter, who visited and described the Nagpur mines, object to this assertion, and endeavour to prove that the "diamond sandstone of the Southern Marathá country is a conglomerate reposing upon the arenaceous beds, which have *never* yielded the precious stone, nor are there any data to prove that the conglomerate derived most of its materials from that source." Dr. Heyne contributed an excellent description of the mines of Southern India, especially those of Banaganpilly, of Ovalumpilly (6 miles from Cuddapah), and of others on the Ellore district. This experienced geologist concludes that all these diamond mines can be considered as nothing else than alluvial soil." Major Franklin ("Geolog. Trans.," 2nd Series, vol. iii., Part 1), who visited the mines of Pannah in Bandelkhand, before Victor Jacquemont's day, makes the diamond sandstone between the Narbada (Nerbudda) and the Ganges belong to the "New Red," apparently an error; and others have described the diggings east of Nagpur (Central Provinces) as having been opened in a matrix of lateritic grit. Dr. Carter ("Summary of the Geology of India," pp. 686-91) connects the "diamond conglomerate" with the Oolitic series and its *débris*, and he gives a useful tabular view of the strata in the mines of Banaganpilly, described by Voysey, and Pannah or Punna by Franklin and Jacquemont. The most important conclusion is their invariable connection with sandstone.

Dr. Carter's volume quotes largely from the writings of Mr. Voysey (Journal As. Soc., Bengal; Second Report on the Government of Haydarábád), a geologist who maintained the growth of the diamond as others do of gold: he declared that he could prove, in alluvial soil, the re-crystallisation of amethysts, zeolites, and felspar. During his last journey from Nagpúr to Calcutta, he visited the diamond washings of "Sumbhulpore" in the Mahanadi Valley, and he describes the gems as being "sought for in the sand and gravel of the river, the latter consisting of pebbles of clay-slate, flinty slate, jasper, jaspery iron stone of all sizes, from an inch to a foot in diameter."

We possess, fortunately, a modern description of the Diggings which I have said were visited successively by Major Franklin and by Victor Jacquemont. M. Louis Rousselet ("L'Inde des Rajahs," Paris, Hachette, 1875), in his splendid volume, gives an illustration and an account of the world-famous mines of Pannah, the Pannasca of

Ptolemy (?), a little kingdom of eastern Bandelkhand, erected in 1809. The Rajah sent a jemadar to show him the Diggings, which are about twenty minutes' walk from the town. The site is a small plateau covered with pebble-heaps, and, at the foot of a rise somewhat higher than usual, yawns the pit about 12 or 15 feet in diameter by about 180 feet deep. It is pierced in alluvial grounds, divided into horizontal strata, *débris* of gneiss and carbonates, averaging 13 metres: at the bottom is the diamond-rock, a mixture of silex and quartz, in a gangue of red earth (clay ?). The naked miners descend by an inclined plane, and work knee-deep in water, which the Noria or Persian wheel turned by four bullocks is insufficient to drain; they heap the muddy mixture into small baskets, which are drawn up by ropes, whilst a few are carried by coolies. The dirt is placed upon stone slabs, and sheltered by a shed; the produce is carefully washed, and the siliceous residuum is transferred to a marble table for examination. The workmen, each with his overseer, examine the stones one by one, throwing back the refuse into a basket: it is a work of skill, as it must be done with a certain rapidity, and the rough diamond is not easily distinguished from the silex, quartz, jasper, hornstone, &c.

Tradition reports that the first diamonds of fabulous size were thus found, and the system of pits was perpetuated. When one pit is exhausted it is filled up, and another is opened. The system is bad, as 100 cubic metres must be displaced to examine one, and around each well a surface of twenty times the area is rendered useless. Moreover, much time is lost by the imperfect way of sinking the shaft, which sometimes does not strike the stone.

This diamond stratum extends more than 20 kiloms. to the north-east of Pannah; the most important diggings are those of the capital, of Myra, Etawa, Kamariya, Brijpur, and Baraghari. The mean annual produce ranges between £40,000 and £60,000—a trifling sum, considering that the stones are the most prized and sell for a high price. They are pure and full of fire; the colour varies from the purest white to black, with the intermediate shades, milky, rose, yellow, green, and brown. Some have been found weighing 20 carats, and the Myra mine yielded one of 83, which belonged to the Crown jewels of the Mogul. The real produce must be taken at double the official estimate. The Rajah has established an approximate average amount, and when this descends too low he seizes one of the supposed defaulters and beheads him or confiscates his goods. He sells

his diamonds directly to Allahabad and Benares, and of late years he has established *ateliers* for cutting, fitted with horizontal wheels of steel worked by the foot.

Evidently here we have a primitive style, which has not varied since diamond-working began. Good pumps are required to drain the wet pits. Instead of sinking a succession of shafts, tunnels should be run along the veins of diamond-bearing rocks. Magnifying glasses and European superintendence would improve the washing. Evidently the yield would double in the hands of Brazilians or South-Africans.

The precious stone is still brought for sale from the nearer valley of the Krishna to Haydarábád: it occurs, I was assured, in a whitish conglomerate of lime locally called Gar-ka-pathar, which must be broken up and washed. During my week's visit I was consulted by two Parsee merchants concerning the rudimentary tests of scratching and specific gravity. In fact, at Golconda, where the finest gems used to be worked, no one, strange to say, can now recognise a rough diamond.

In the "Highlands of the Brazil" (ii., 113) I have given a detailed list of the various stones associated with the gem, and specimens of the Cascalho or diamond gravel, the Taua, the Canga, &c., have been sent to the Royal Society of Edinburgh by Mr. Swinton. It is advisable to remark that this association has everywhere been recognised. In Borneo we are told that "the diamond is known by the presence of sundry small flints." The gem-yielding pebble-conglomerate of India, not usually a breccia, as was proved by Franklin Newbold and Aytoun (*loc. cit.*, p. 386), contains quartz and various quartzose formations; garnet, corundum, epidote and Lydian stone; chalcedony and cornelian; jasper, of red, brown, bluish, and black hues; and hornstone, a kind of felspar, whilst "green quartz indicates the presence of the best stones." Fossil chert is yielded by the limestone, and the highly ferruginous and crystalline sandstone produces micaceous iron ores, small globular stones (pisoliths ?), and almost invariably fragments of iron oxide. Finally, there are generally traces of gold, and sometimes of platinum. At Haydarábád I was assured that such was the case on the Krishna River, but none of my informants had any personal knowledge of washing. Dr. Carter's "Geological Papers" convinced me that the sandstones of the diamond area will be found to resemble the "Itacolumite," quartzose mica slate or laminated granular quartz, of Brazilian "Minas Geraes."

These considerations persuade me that diamond-digging

in India generally, and especially in Golconda (the territory of Haydarábád), has been prematurely abandoned. In the seventeenth and eighteenth centuries the machinery for draining wet mines was not what it is now, and the imperfect appliances led to the general belief that all the deposits were purely superficial. Doubtless some of the deposits were in the alluvial soil of the most recent rocks, but M. Rosselet's account shows that deep digging may still be practised to advantage. Voysey also saw the "sandstone breccia" (diamond conglomerate ?) of Southern India "under 50 feet of sandstone, clay slate, and slaty limestone." The Brazilian miners ("Highlands," ii., 121) have only lately learned to descend 180 feet, and they find some of their best stones at the lowest horizon. The Vaal River and other South African washings, opened in 1868, soon reached 60 feet.

I had heard of chance diamonds being picked up by the accolents of the Krishna River, and Sir Salar Jung, with his usual liberality, proposed laying a dák for me to Raichor; he was ready, in fact, to meet my wishes in every possible way. I presently, however, learned from good authority that only crystalline rocks like those which I had seen in the Golconda tombs are produced by this central section of the Krishna, and that "Itacolumite" must be sought elsewhere. Evidently the precious stones have been rolled down from some unknown distance, and to follow the "spoor" demanded more time than I could command.

It is useless to insist upon the benefits of reviving the ancient industry. Haydarábád is not a rich country, and her trade is well nigh *nil*. But she has coal that wants only a market, and if to the "black diamond" she can add the white diamond, her future prospects are not to be despised. The first step is, of course, that of "prospecting," of systematically reconnoitring the ground, with the aid of a few experienced hands imported from the Brazils and South Africa. If the search be successful a company or companies would be soon found to do the rest. For me it will be glory enough to have restored the time-honoured "mines of Golconda."

We left at the week's end the country of "our faithful ally," greatly pleased with the courtesy and hospitality which seem to be its natural growth. And I have a conviction that, despite the inevitable retrograde party of all native states, the *codine* of the East,—the warlike Zemindars, the "dissolute vagabonds," the "Pathan bravos," and the "cut-throats and assassins" of the Press,—this realm has become since 1857 the "greatest Mohammedan power in India."

The return journey to Bombay gave time for other reflections. At present our "enormous dependency, India, the most populous and important that ever belonged to a nation, and conferring a higher *prestige* on the ruling race than has ever been conferred by any other subject people,"—as the judicial Trollope has it,—is, has been, and under present circumstances ever will be, somewhat neglected by the general public of England. No home Britisher can interest himself even moderately in such a colony. It is too distant, and it can hardly be brought nearer by local parliaments and similar institutions. Although "taxation without representation is tyranny," we are not yet prepared to grant what eventually must be granted, representative government. We are therefore driven to seek some other course.

At Haydarábád, as in India generally, we are living upon a volcano, which may or may not slumber for years. The remedies hitherto proposed for the natural disaffection of the great native powers, kept as they are in a state of quasi-tutelage, appear to be mere quackeries, likely to do harm rather than good. For instance, to make the energetic Indian Prince more powerful within his own jurisdiction would be simply to arm him against ourselves.

But why not at once admit a certain number to seats in the House of Lords? Of those who claim salutes of 21 guns there are, besides four foreigners, three Indian Princes,—the Nizam, the Gaikwár, and the Ruler of Mysore,—who all happen at present to be minors. Amongst those honoured by 19 guns we find Scindhia, Holkar, and Udepúr; whilst Jaipúr, with twelve others, has 17 guns. Of course it would be necessary to limit the number to six or seven, but the hope of eventually rising to the dignity should not be withheld from the chiefs of lower grade.

Nothing would tend more directly to conciliate the Princes of India, and to make them our firm friends, than to admit them to the highest dignity of the Empire,—to a House where they would doubtless hasten to sit, where they would learn their true interests, and where they would find themselves raised to a real instead of a false equality with the ruling race.

VI. CERTAIN PHASES OF BIRD-LIFE.

By Charles C. Abbott, M.D.

NOTWITHSTANDING so general an interest has been taken in studying the habits of our birds, by both scientific and amateur naturalists, there are several phases of bird-life to which little or no attention has been paid; at least scant reference, if any, has been made to them in ornithological literature.

One such feature of bird-life is the mode of acquiring the range of flight-power characteristic of each species. A careful and long-continued study of our birds in their chosen haunts, free from all unnatural (*i.e.*, human) persecution, has enabled me to detect but little variation in the flight-powers of the individuals of any species of bird observed—far less than in the general range of their habits; but still such individual variation, I think, does exist. A bird is not a perfectly-adapted machine, capable of faultlessly filling its destined place in Nature, and unerringly performing everything required of it. With the simple growth of the feathers of the wing there does not come the ability to fly. Just as creeping precedes walking, in children, this is a gradually-acquired power. The commencement may be termed "flapping," and consists in simply breaking the force of a descent; this is followed by a more effectual use of the wings, and horizontal progression, and it is some time subsequent to this that the young birds attain to the power of *upward* flight. This holds good of a considerable number of species, studied with special reference to their flight—as the robin, the wood-thrush, cedar-bird, cat-bird, pewee, and indigo-bird.

It is doubtful if young birds, while yet in their nests, are conscious of the use to be made of their wings. After long-continued experimenting, I find they make no use of them in endeavouring to escape, but trust to their legs entirely if removed from the nest, or defend themselves by pecking at the intruder. When a sufficient growth of feather has been obtained the parent-birds, directly and indirectly, instruct them, or, perhaps more properly, force them to use their wings. So, at least, I can only interpret certain habitual actions of the parent birds with reference to their newly-fledged young.

As an instance I will quote from my field-notes, with reference to the indigo-bird :—"June 23, 1873. Found a nest of this species in a dense thicket of blackberry, and, curiously enough, within just seven paces of the railroad-track. The young birds were just ready to leave the nest. I visited the nest the next day, and saw on my approach one of the four young birds sitting on a brier-stem, about a yard from the nest. Taking a favourable position, I continued to watch the birds closely, as they were very restless and noisy. Evidently something unusual had occurred or was occurring. In a few moments I saw the hen bird go to the nest, and push one of the young birds out of the nest. It forced it from the edge of the nest, to which it clung with its feet. Once free, the little fellow struggled to keep itself up, throwing *up* its wings, as a child would straighten out its arms when falling. This was the initial movement that developed into flight. All of the young birds were thus forced from the nest, and I am satisfied from no outside cause, as, for the three following evenings, the young returned to the nest to roost. I spent several hours watching this brood and their parents, and the whole time was occupied, except short intervals when they were fed, in forcing the young birds from point to point, but ever keeping them from the railroad-track, over which trains passed frequently. Two days from leaving the nest they could fly 6 or 8 yards, but always from a higher to a lower perch, and regained the more elevated branches by very short, 'jumping' flights, with a laborious flapping of the wings ; but on the fifth day they could follow their parents almost any distance, and execute an upward flight with apparently the same ease. Examination of the wing-feathers on the 30th of June, as compared with a week previous, showed so slight gain in the growth of the feathers that I believe nothing in the increased flight-power was due to their being now better fledged."

Such observations as the one noted in detail I have so frequently repeated with widely-differing species as to satisfy me that what may be termed "direct instruction" in flight is given to the young birds by their parents. "Indirect instruction" also is noticeable, in the fact that the parent-birds cease to feed their young, and so force the latter to leave the nest and follow them. Once out of the nest they soon endeavour to walk on air, as it were, and, falling, open their wings, and, as described, thus take the initial step. This ceasing to bring the food to the young while yet in the nest is done in some instances, I judge, only to draw them

from the nest; and then they feed them as before, but not as frequently, which leads the young to voluntarily move from point to point. The important fact must not be lost sight of, too, that the young birds, when once out of the nest, witness nearly every movement of their parents, and learn, undoubtedly, very much through imitation of their movements.

For these reasons I believe the acquisition of full-flight power is gradually acquired: first there is a mere "flapping" to prevent falling; then short horizontal stages of aërial progression; finally, a steady, intelligent use of the wings, enabling the birds to execute the highest type of flight within their capabilities, *i.e.*, upward flight.

In the case of birds of more complicated flight than those mentioned above, such as the falcons, where hovering is a necessary acquirement, the truth of the assertion that flight is gradually acquired becomes more evident from the fact (which I have very frequently verified by observation) that the young birds for some time after leaving the nest are fed by their parents. They commence procuring food for themselves by chasing sparrows; checking their moderate flight when above a thicket, they rush upon the fleeing birds, more frequently without success than with. Their first attempts at hovering are miserable failures, and it is not until autumn that they are enabled, by the complete control of their wings, to stay themselves in mid-air, and, at the proper moment, dart with unerring aim upon some luckless mouse.

I have used the term "unerring," because it is customary so to characterise this act of the falcons; but having watched, with a powerful field-glass, the hovering and darting of hawks, I have been forced to consider the term far from correct, and that not more than one-half, if as many, of the "strikes," on the part of the bird, are effectual.

Following the young birds, of any species, from the nests, and noting their movements, we find that the one prominent aim of their lives, during their first summer, is the acquisition of food. They have really nothing else to do, if we except escaping from the attacks of their enemies, and this is taught them directly by their parents. I judge that the great majority of birds that fall victims to birds of prey and carnivorous mammals are young. To return to the feeding-habits of birds:—These appear to be acquired, by every bird, through imitation of the movements of the parents when in search of food; judgment as to localities, on the part of the young, and allied circumstances connected with

procuring food, come by experience. Watch a restless little creeper (*Certhia familiaris*), during chill winter mornings, as it flies from tree to tree and clambers over and about the rough bark. It seems, indeed, a mere automaton, driven, and not going of its own free will; but, if we continue our observations but a little longer, we shall find it really a discriminating creature, passing by certain trees that are to us all one with those visited. It is not chance, but a consciousness of the uselessness of search, that determines its flight to some more distant rather than a nearer tree.

As an example of the knowledge gained by young birds through imitation, let us take young woodpeckers. On leaving the nest they accompany their parents, but are not fed by them. Like the old birds, they immediately commence to climb the trunks and branches of the trees. Having been fed with insects when in the nest, they are already able to recognise their proper food, and devour the visible insects they may discover on the outer surface of the bark. Now, was it the example set by their parents, or the peculiar construction of their bill and feet, that was the cause of their having sought the trees and climbed over them in the peculiar manner common to their kind? I think, clearly the former. Now, merely clambering over the bark of trees would not enable them to secure sufficient food, and imitation could not extend beyond this point; but here experience comes into play, and the gradual acquirement of the whole routine is easily traced. The bark of trees is nearly always cracked, and in the crevices are more insects than on the surface, and the habit, soon acquired, of search in the cracks of the bark is the one step from searching over the exposed surface to search beneath. Imitation led the *ignorant* young bird to the thrifty growth of timber, and not to the tangled hedge-rows. Experience taught him the accustomed haunts of those insects on which Nature bids him prey. If we go back into the remote past, and recal the ancestral woodpecker, we can, with no undue use of the imagination, picture to ourselves the first steps that led the good climber to find in the half-decayed bark the nourishing food abounding there; and now let us return to the present, and seek for some variation in the habits of the birds of to-day. As an instance, the "flicker," or golden-winged woodpecker, leaves the timber-lands, and in loose flocks, often in company with robins, wanders over pasture-fields and meadows in search of food, more especially the crickets, and not under fences do they look for them,

but under the dry droppings of the cattle. Here is an instance where accident, it may be, gave origin to—and experience has confirmed into—a habit, a decided variation from normal woodpecker life. Now, a young woodpecker leaving its nest June 1st, if dissociated from its kind, would never leave the woodlands, and, seeking the pasture-fields, overturn dry chips of cow-dung, in search of crickets; but such young birds will naturally follow their parents thither,—and this is just the case, for the larger proportion of birds killed in October, in such localities, are the young of the preceding summer.

In conclusion, with reference to young birds, I believe they leave their nests *totally ignorant*, and *naturally* imitate their parents. What this imitation secures to them, in the way of knowledge, they perfect by experience; and this explains the variation in the habits of the same birds, so noticeable when studied in localities widely distant and greatly differing in character.

Let us turn our attention now to adult birds; and, with reference to them, I would refer particularly to two phases of their life-habits that have interested me exceedingly. The first of these points is the ingenuity so frequently displayed in procuring food. By the exercise of ingenuity, I mean instances of the attacking bird (in cases of birds of prey) being at first outwitted by the pursued, and, after repeated efforts availing nothing, ceasing its aggressive movements; then considering the causes of failure, planning a new method of action, and, having correctly judged the difficulties, finally succeeding. This, at least, is the manner in which I interpret the following instance:—

While out watching our winter birds, January 22nd of this year, I was caught in quite a hard shower, and sought shelter under a group of three large, dense cedars. Like myself, driven in from the adjoining meadows by the increasing rain, came a dozen or more sparrows, which, settling among the branches, commenced dressing their feathers and twittering cheerily. In a few moments after came, with a rush and loud chirp, a gay cardinal. If the sparrows did not acknowledge his presence with a low bow, each, at any rate, took a lower branch, leaving him on his elevated perch like a monarch on his throne. But he was fated to be molested, for, scarcely had he become fairly settled, and his feathers smoothed, when a sparrow-hawk rushed through the tree, with a zigzag movement, endeavouring to seize him or one of his attendant sparrows. Failing in this, the hawk hovered about a few moments,

giving the scattered birds time to return, which they quickly did, when, with a similar rush, he again scattered them. One little snow-bird was so thoroughly frightened that it lit upon my shoulder, as though seeking safety under the brim of my hat. The third effort of the hawk failing, he came back immediately and seated himself at a little distance from the top of the tree, and close to the main stem. I remained nearly motionless, but with upturned face, and could plainly see the bird, though fortunately I escaped notice. One thing in particular attracted my notice; the bird was very much exhausted—"out of breath," as we should say of ourselves—and, with his beak open, he panted violently. This satisfied me that the efforts to capture prey are not accomplished with the ease sometimes supposed. As the rain was increasing, and the wind considerable, the sparrows again collected in the tree; and now the hawk rushed *out* instead of in, and bore a luckless sparrow in his claws.

I think that we have here all that I claimed, when speaking of ingenuity on the part of adult birds in seeking their food. There was in the above instance a painful consciousness, at first, of failure to secure the desired prey; there was a determination to succeed, in spite of failure at the start, and a correct determination of the cause of failure, coupled with the invention of a plan by which the difficulties might be overcome. What more should be required to demonstrate that the mental powers of lower animals differ from those of man solely in degree?

Again, let us consider a case of ingenuity displayed by a bird in successfully avoiding an enemy. Here there is more cause to be surprised at the result, inasmuch as there was no cessation of the attack to give the pursued bird time for considering how best to act under the circumstances; but, while fleeing for life, it matured a plan of escape that happily succeeded. This instance of ingenuity on the part of a pursued bird I have already related ("Land and Water," March 2, 1872), but, considering it more remarkable than any other that has occurred to my knowledge, and having witnessed a repetition of it two years later, I again relate it in preference to other instances I have noted bearing upon the same subject. The case is that of a "king-rail" (*Rallus elegans*), which my spaniel flushed in open ground, the grass not being tall enough to conceal it. The bird trusted wholly to running, and kept clear of the dog; frequently it "doubled," and seemed to enjoy the chase; but, evidently becoming somewhat fatigued, as shown by the nearer

approach of the spaniel, it ceased doubling, but, running in a straight line some distance, it allowed the dog to get within a foot or more, when it jumped, with a single flap of its wings, a foot or more from the ground; then dropping down quickly behind the dog, it turned and ran in the opposite direction, gaining considerable ground before the impetuous spaniel could check its speed, turn about, and follow. Here, again, as we would express it in describing any human experience, "the circumstances of the case were taken into consideration" by the pursued bird, and, without taking to flight, as would seem the more natural act, it surmounted the difficulties and effected its escape. I can conceive of no other way of explaining this action of the rail-bird than by admitting that a train of thought passed through the brain of the bird—that it thought, "If I gain time, I am safe," just as any pursued person would think that, if he could reach some spot, be heard, &c., he would be safe. And, while yet running at great speed, the bird *thought* of an ingenious plan by which it *did* gain time and reached the reedy creek-bank in safety.

It might be argued that a single act of a bird at some one time and under peculiar circumstances does not constitute a habit—that it simply chanced to do so and so; but a second occurrence of the kind would result differently. It must be remembered, however, that where a bird is noticed in its natural haunts once, even if for more than an hour—which is an unusually long observation—there are weeks when this same bird is unseen, and therefore what its acts may be are absolutely unknown. For this reason, an ingenious act of a bird may be frequently repeated, and almost certainly is. Indeed, our ignorance of bird-life is so great, that what seem to us "curious instances," because but seldom witnessed, are frequently daily occurrences and ordinary features of the bird's life. It can scarcely have escaped the notice of close observers of our winter birds that their comparative abundance is only during clear, pleasant weather, when they will be as lively and restless as spring birds in early summer, and that during the winter certain localities, as the southern outlooks of wooded hillsides and such sheltered spots, are those where these hardy species "most do congregate." During a mild day at some such spot we can almost delude ourselves into thinking that spring is coming; but on the morrow a fierce wind rattles the bare branches above you, clouds of stinging dust or driving snow fill the chilled air, and not a bird is to be seen or heard, the cheery sights and sounds of yesterday having given place to a dreariness

most drear. One question now arises, and we naturally ask, "What has become of the birds so lately here?"

During the winter of 1874-75 (the coldest except one—1835-36—since 1780) I endeavoured to determine to what extent these birds sought shelter, and the character of it, not only as a protection against severe storms, but as regular winter quarters—*i.e.*, for roosting-places. I was led to do this from the fact that these winter residents, as the bluebird, the cardinal redbird, and the titmouse, do not roost in the trees, as in summer, and it seemed probable that, seeking warmer quarters in ordinary weather, they should seek shelter from severe storms, and not temporarily migrate to some point beyond the limits of the storm; not only this, but that some spot is selected early in the season as such roosting-place and refuge, and occupied as such throughout the season. So far as my observations extend, I was correct in my surmises.

I have on my farm a deep "gully," or ravine, thickly wooded, and with overhanging banks, extending a considerable portion of the entire length. This overhanging earth is held in place partly by the character of the soil, and more by the roots of the trees growing near the margins of the gully. In this locality, under the overhanging earth, in some instances at a distance of 3 feet from the open ground, I found the snow-birds, song and chipping sparrows, occasionally a flock of cedar-birds, the arctic snow-bird, and horned larks roosting; and, judging from the amount of excreta upon the ground, this had been the accustomed roosting-place for many weeks. A little before sundown, during January, I would find these birds, some or all of of them, congregating in the adjoining fields and in the trees of the gully, and quite suddenly they would all disappear. Searching every possible hiding-place, I finally found them as above described. If the following day proved very cold or stormy, many of them would remain in their snug retreat, the arctic visitors being the first to venture out. The birds just mentioned all build open nests, either in trees or upon the ground. On the other hand, the titmouse, nuthatch, brown tree-creeper, and bluebird, all of which build nests in hollow trees or sheltered spots of that character, I found regularly roosted in the hollow trees or in the outbuildings of the farm. The cardinal redbirds, however, which nest in trees and brier-patches, usually took refuge in dense cedars to roost, but sought other shelter during severe storms. For instance, during the remarkable wind-storm of February 9th, when the air was filled with dust, and the thermometer

ranged from 3° to $4\frac{1}{2}$° F., no ordinary shelter could protect our resident birds. During the day not one was to be seen flying. I found the cardinal redbirds—a pair of them—had taken shelter in a large hollow tree, and with them was quite a large number of titmice, a brown tree-creeper (*Certhia familiaris*), and several sparrows. I do not doubt but that the earth-shelter already described had proved inadequate, and that the birds usually roosting there had sought more sheltered spots. However, I did not have the courage to face the wind and see for myself if such was the case.

During the present winter I have found that some, at least, of our winter birds utilise the very excellent shelter afforded by the nests of our bank-swallows. February 20th, a bright, clear day, I passed by a high, steep cliff of compact sand and clay, much frequented by these swallows during summer. I noticed there chipping-sparrows and a bluebird sunning themselves, each at the opening of a nest. On driving them away I found that they circled about for a few moments and returned. On passing the cliff again some hours later, I saw these birds and several others, some at the openings of the nests, and others flitting about, quite in the manner of swallows. I could not reach the nests to determine if they had been much occupied during the winter, but do not doubt but that such was the case.

I have not found, however, any shelters constructed by birds for such purpose solely, except in the case of the introduced English sparrow, which builds quite an elaborate and very warm roosting-nest. During the early frosts of autumn and prevalence of cold rain-storms occurring before the ordinary date of migratorial departure, the nests used in spring and summer are, I know, used as roosting-places, but I have not detected any refitting of them for this purpose. Considering this, it would be natural for birds to build new structures for winter use, and in the sparrow we have an instance of it, and, I presume, the abundance of natural shelter has alone prevented the gradual acquirement of this habit by our winter birds.

Having familiarised one's self with the various phases of bird-life as it occurs in the open fields, dense thickets, along secluded streams, and in shady forests, one can scarcely conclude otherwise, if happily he has not entered upon his studies with some preconceived notion, than that these wild and wary falcons, timid sparrows, fiery little wrens, and cautious waterfowl are creatures that, like man himself, are thrown upon the world dependent upon their own exertions,

guided by their own reasoning powers. There are no pre-arranged rules which, when birds leave their nests, they must strictly follow to exist. Given that knowledge which comes through direct and indirect instruction from the parent-birds, and a young bird, having the world before it, exercises just those mental powers that man exercises, but limited just so far as its own wants are less than man's wants as man. Finally, in the chance occurrence of some peculiar habit, have we not a trace of the former mode of life of some far-distant ancestral form; and, in the undeniable irregularity of all habits, can we not discern unmistakable indications of the gradual adoption of every habit, just as the various specific forms themselves gradually emerged from the archaic creature that, appearing in the dim past, foreshadowed the gigantic condor of the Andes, and the petulant humming-birds of our summer gardens?

VII. THE LOAN COLLECTION OF SCIENTIFIC APPARATUS AT SOUTH KENSINGTON.

THE English Government seems, at last, as if it were becoming alive to the fact that the wealth and commercial prosperity of the country are, to a large extent, due to the discoveries and researches of scientific men. While the respective Governments of America, France, Germany, and other countries have established scientific universities, and recognised in numerous ways the claims of scientific workers, the prosecution of all branches of scientific research in England has been almost entirely left to private enterprise. But we suppose we may take the present Exhibition of Scientific Apparatus as a proof that Science will henceforth have its position acknowledged in the deliberations of the Government of England, and receive some small share of the money expended for the honour and welfare of the country. The great success of the present Exhibition has been widely proclaimed by the whole of the public press. Some journals have, it is true, doubted the advantage of bringing from a distance instruments of mere historic value: if, however, they are the means of recalling to the minds of our legislators and the public generally the

splendid discoveries of the men who used them, and the part they played in the advance of civilisation, then, indeed, were this the only good result of the Exhibition, it would have served a great and important end. Had no adventitious means been adopted to arouse the interest of the public in the various instruments, the practical value of the Exhibition—excepting to scientific men—would, perhaps, have been an open question; but it is only necessary to attend the evening lectures which have been so kindly undertaken by eminent scientific men, and notice the eagerness with which the large audiences listen to the lectures, to be convinced that the object in visiting the Exhibition is the desire for knowledge, and not mere idle curiosity. The demonstrations, too, were attended by considerable numbers of people, and whatever instrument happened to be under demonstration—whether Radiometers, or Dr. Siemens's Bathometer and Attraction Meter, or M. Pictet's machine for the production of Artificial Ice—questions were freely asked, until the construction and mode of action of the instrument were understood. In the present number we have only sufficient space to refer briefly to the more important of the historical instruments used in chemical and physical researches.

Chemistry.

In the Chemistry Section there is exhibited the apparatus employed by John Dalton in his Researches. The following description of this Series has been contributed by Prof. Roscoe, F.R.S. :—

The apparatus employed by John Dalton in his classical researches, whether physical or chemical, was of the simplest, and even of the rudest, character. Most of it was made with his own hands, and that which is to be exhibited has been chosen as illustrating this fact, and as indicating the genius which with so insignificant and incomplete an experimental equipment was able to produce such great results. The Society has in its possession a large quantity of apparatus used by Dalton, most of which, however, consists of electrical apparatus, models of mechanical powers, models of steam-engines, air-pumps, a Gregorian telescope, and other apparatus of a similar kind, which was either bought or presented to him. It has not been thought necessary to exhibit these, but rather to show the home-made apparatus with which Dalton obtained his most remarkable results.

I. *Meteorological and Physical Apparatus made and used by Dr. Dalton.*

Throughout his life Dalton devoted much time and attention to the study of meteorology; indeed his first work, published in 1793, was entitled "Meteorological Observations and Essays," and his last paper, printed in 1842 (Mem. Lit. and Phil. Soc., vi., 617) consists of auroral observations. Hence the first of Dalton's apparatus which claim attention are the meteorological instruments.

No. 1 is Dalton's mountain barometer, with accompanying thermometer, made for him by the late Mr. Lawrence Buchan, of Manchester. The barometer is enclosed in a wooden case, which Dalton was accustomed to carry in his hand.

Several home-made barometers used by Dalton in his observations are in possession of the Society. They are all of them filled, and the scales prepared, by Dalton himself, and are simple syphon tubes with a bulb blown on at the bottom to serve as a mercury reservoir. These are attached to plain pieces of deal, upon the upper part of which the paper scale is pasted. One of these, which has probably also served for tension experiments (No. 2, has been placed in the collection.

Many of the thermometers appear also to have been home-made.

No. 3 is a mercurial thermometer, evidently made and graduated by Dr. Dalton, and marked with his initials, J. D. The freezing-point of this thermometer was tested recently by Mr. Baxendell, who found that it had not altered since the instrument was graduated.

Another (No. 4) is of the same kind, and bears date 1823.

No. 5 is a third mercurial thermometer with long stem and wooden scale.

No. 6 is an alcohol thermometer with wooden scale.

No 7, a registering maximum and minimum thermometer employed by Dalton; maker's name, J. Renchetti, 29, Balloon Street, Manchester.

II. *Apparatus constructed and used by Dalton in his Researches.*

(1) "On the Constitution of Mixed Gases," (2) "On the Force of Steam or Vapour from Water or other Liquids at Different Temperatures, both in a Torricellian Vacuum and in Air," (3) "On Evaporation," and (4) "On the Expansion of Gases by Heat" (by John Dalton, "Mem. Lit. and Phil. Soc. of Manchester," 1st series, vol. v., part 2).

No. 8 is an apparatus used for the determination of the tension of volatile liquids at low temperatures; it consists of a syphon tube, at the upper end of which is a scale in inches in Dalton's handwriting. He describes it thus:—"I took a barometer tube 45 inches in length, and, having sealed it hermetically at one end, bent it into a syphon shape, making the legs parallel, the one that was closed being 9 inches long, the other 36 inches. I then conveyed two or three drops of ether to the end of the closed leg, and filled the rest of the tube with mercury, except about 10 inches at the open end. This done, I immersed the whole of the short leg containing the ether into a tall glass containing hot water."

No. 9 is a smaller tube containing another liquid, also having a graduated scale written on paper and attached to the tube.

Nos. 10, 11, 12, 13, 14, are tubes used by Dalton for measuring the tension of vapour from water and other liquids at higher temperatures than their boiling-points, both in a vacuum and air.

No. 15 is a tube used by Dalton for measuring the tension of the vapour of bisulphide of carbon, labelled "Sulphuret carb.," with a paper scale in Dalton's handwriting, and a cork showing that the upper portion of the tube containing the bisulphide of carbon could be heated in a water-bath to various temperatures.

No. 16 is a manometer tube, fixed into a board, divided and numbered by Dalton.

No. 17 is an apparatus used by Dalton for the determination of the tension of the vapour of ether, and is interesting as being the instrument by means of which Dalton arrived at one of his most important experimental laws. It is described as follows (p. 564):—"The ether I used boiled in the open air at 102°. I filled a barometer tube with mercury moistened by agitation in ether; after a few minutes a portion of the ether rose to the top of the mercurial column, and the height of the column became stationary. When the whole had acquired the temperature of the room (62°) the mercury stood at 17·00 inches, the barometer being at the same time 29·75 inches. Hence the force of the vapour from ether at 62° is equal to 12·74 of aqueous vapour at 172° temperature, which are 40° from the respective boiling-points of the liquids." This is generally known as Dalton's law of tensions, since shown by Regnault not to be rigorously true.

No. 18 is a wet and dry bulb mercurial thermometer made by H. H. Watson, of Bolton.

III. *Apparatus for measuring Gases and for determining the Solubility of Gases in Water.*

No. 19 is an apparatus with a graduated tube, probably used by Dalton for the determination of the laws regulating "The Absorption of Gases by Water and other Liquids," read October 21st, 1803 ("Manchester Memoirs," 2nd Series, vol. i.).

No. 20 is a graduated glass tube attached to a bottle of india-rubber, also probably used in his researches on the absorption of gases by water.

No. 21, No. 22, are divided eudiometer tubes, employed by Dalton for measuring the volumes of gases.

No. 23 is a spark eudiometer.

Nos. 24, 25, 26 are glass tubes, pipettes, and funnels, graduated by Dr. Dalton and used by him for measuring gases.

No. 27 is a graduated glass bell-jar, used for measuring gases.

No. 28 is a phial, with graduated tube attached by cement, for collecting and measuring gases.

Nos. 29, 30 are stoppered phials, with the bottoms cut off, used as gas jars for collecting and measuring gases.

No. 31 is 1000 grains specific gravity bottle, with its counterpoise of lead stamped "175" by Dalton, and paper labelled in his handwriting "bottle balance."

No. 32 is a pipette.

No. 33, square bottle of thin glass, fitted with brass caps, and probably used for the determination of the specific gravities of gases.

No. 34 is an earthenware cup, used by Dalton as a mercury-trough, and containing a small phial with mercury.

Nos. 35, 36 are bulb tubes, with graduated scales, serving for the determination of the coefficients of expansion of gases.

No. 37 is a Florence flask with cork and valve for determining the specific gravity of gases.

No. 38 is a glass alembic.

IV. *Weights, Balances, Apparatus, Reagents, and Specimens used by Dalton.*

No. 39, eleven phials, containing creosote, iodine, amalgam of bismuth and mercury, quercitron bark, grana sylvestra, cochineal, and other substances, labelled in Dalton's handwriting.

No. 40, three divided blocks, used by Dalton for the illustration of his lectures: these are not, however, the balls an

inch in diameter (referred to in his latest memoir on the "Analysis of Sugar") which he employed occasionally in his lectures, as illustrating his newly-discovered laws of combination and the atomic theory; these appear, unfortunately, to be no longer in existence.

No. 41 is a common pair of scales used by Dalton.

No. 42, a pair of apothecary's scales and weights employed by Dalton, with a paper of weights made of wire, labelled in his handwriting "100th grains."

No. 43 is a box of weights used by Dalton, and containing a pill-box labelled "Platina," another pill-box labelled "Hund," and containing 100th of grains, and another wooden box containing brass gramme weights, labelled "Weights, French;" the other ordinary weights are of lead.

No. 44 is Dalton's pocket-balance, consisting of a small pair of apothecaries' scales, with beam about 4 inches long, and having the pans attached by common string; it is contained in a tin case for the pocket.

No. 45 is a penholder used by Dalton.

No. 46, leaden grain weights made by Dalton from sheet lead, and stamped in numbers by him.

No. 47, iron punches used by Dalton for this purpose.

No. 48, a glass lens, wrapped in a piece of paper, labelled in Dalton's writing "Sun's focus 4·2 inches."

No. 49 is a paper containing "10th of grains," made by Dr. Dalton of iron wire. The paper in which these are wrapped is part of a note from one of Dr. Dalton's pupils (as is well known he lived by teaching mathematics at half-a-crown per lesson), in which the writer presents his "compliments to Mr. Dalton, and is sorry that he will not be able to wait upon him to-day, as he is going to Liverpool with a few friends who are trying the railway for the first time. Mr. D. may fully expect him on Monday at the usual time."

No. 50 are bottles of tin, earthenware, and silver, some of them being common penny pot ink-bottles. Each has a thermometer tube cemented into the neck of the bottle, and these tubes are provided with paper scales. These were used by Dalton probably for experiments on radiant heat.

No. 51 is a manometer tube used by Dalton: it consists of a tin vessel attached on either side to leaden tubing, and having a thermometer tube closed at the upper end, and provided with a divided scale, fixed into the upper portion of the tin vessel.

No. 52, Dalton's Balance, made by Accum, and capable of arrangement as hydrostatic balance with weights and counterpoises.

This section also contains :—

Balance used by Cavendish.—This balance, of rude exterior but singular perfection, was made by Harrison, according to the plan and by order of Henry Cavendish, and passed at his death to his cousin and heir, Lord George Cavendish. By him it was presented to Sir Humphry Davy, together with the greater part of Mr. Cavendish's philosophical apparatus. Presented to the Royal Institution of Great Britain by Mr. Felix R. Carden.

Balance used by Dr. Thomas Young and Sir Humphry Davy.—A balance made by Fidler for the Royal Institution.

Balance used by Sir Humphry Davy.—Presented to Prof. Roscoe by Mrs. F. Crace-Calvert.

Balance used in his experiments by Dr. Joseph Black, Professor of Chemistry in the University of Edinburgh, from 1766 to 1799.

Pneumatic trough used in his experiments by Dr. Joseph Black.

Glass chemical vessels (retort, bottle, and flask or receiver) used in the chemical laboratory of the University of Edinburgh during the latter half of last century, showing the contrast between them and vessels used for similar purposes at the present day.

Apparatus employed by the late Thomas Graham, F.R.S., Master of the Mint, in his principal researches between the years 1834 and 1866. The series is interesting as showing the simplicity of the appliances with which Graham worked, and by the aid of which he discovered facts and established laws which have since proved to be of so much importance.

Old cupellation furnace, supposed to have been the one used by Sir Isaac Newton, when Master of the Mint, in some experiments on the cupellation of silver.

Touchstone for the assay of gold, formerly used in the Royal Mint.—The method is based on the fact that the greater the amount of gold contained in an alloy, the brighter is the gold-yellow colour of a streak drawn with it on a black ground, and the less is it attacked by pure nitric acid or by a "test" acid. In ascertaining the richness of the alloy under examination its streak is compared with marks drawn with alloys whose richness is accurately known.

Apparatus used by Faraday for the condensation and liquefaction of Gases, consisting of condensing pump and connections, conducting and other tubes, gauges, sealed tubes for containing the liquefied gases, &c.

Original tubes containing gases liquefied by Faraday.

In the department of PHYSICS we find :—

Original apparatus with which Faraday obtained the magneto-electric spark.—A welded ring of soft iron, 6 inches in diameter, 7-8ths of an inch thick, one part covered by a helix A containing about 70 feet of insulated copper-wire, occupying about 9 inches in length upon the ring. The other part covered by a second helix B containing about 60 feet of insulated copper-wire. The helices are separated from each other at their extremities by ½ an inch of the uncovered iron. The iron ring was converted into a magnet by passing a voltaic current through the helix A. This induced an electric current in the helix B, and a small spark was for a moment seen at the carbon terminals.

Faraday's original apparatus for magneto-electric induction by a permanent magnet.—A pasteboard tube is surrounded by a helix C of insulated copper-wire. The diameter of the tube allows a cylindrical bar magnet to pass freely into it. The terminal wires of the helix are connected with a galvanometer. On the introduction of a permanent bar magnet into the helix, and on its withdrawal from it, currents of electricity were induced in the helix which caused a deflection of the galvanometer needle.

Faraday's rotating rectangle, for illustrating the inductive action of the earth.—The wire rectangle provided with a commutator for collecting the currents was attached to a galvanometer, and rotated in the line of the magnetic meridian, the electric current induced in the rectangle deflecting the galvanometer needle.

Various helices, spirals, &c., used by Faraday in his researches on magneto-electric induction, &c.

A magnet made by static electricity, with note by Faraday.—"A magnet made at the Royal Institution by an electric discharge from 70 square feet of charged surface. Present, Sir H. Davy, Pepys Jordan, Bostock, and Faraday."

Portion of the battery used by Sir Humphry Davy in decomposing the alkalies.

Diagrams of magnetic curves, prepared by Faraday.

Coils and helices, used by Faraday in his electro-magnetic researches.

Model frequently used by Faraday during his researches on the rotation of a ray of polarised light by electricity and magnetism.

Block of glass pierced by sparks from an induction-coil. Presented to Faraday by M. Ruhmkorff, 1861.

Glass tubes prepared by Faraday for testing the magnetic and diamagnetic character of gases.—The tubes containing the gas to be examined were suspended in the magnetic field of a powerful magnet, the result being either attraction or repulsion of the tubes as the gases they contained were either magnetic or diamagnetic.

Bars of borate of lead glass, made and used by Faraday, for the action of magnets on polarised light.

The diamagnetic box of Faraday, containing spheres, cubes, and bars of diamagnetic metals; tubes of various liquids, bars of borate of lead, glass, various crystals, cradles, supports, &c., used by Faraday in his researches on diamagnetism.

Siberian loadstone and spark apparatus.—This was the loadstone employed by Faraday in his experiments on magneto-electric induction, from which he first obtained the induction spark.

Glass globes for producing electricity by rubbing with the hand. The globes are caused to revolve by means of multiplying wheels and a band of rope. The globes may be exhausted when they become luminous; the greatest amount of electricity or "fire" was obtained from them when they were exhausted. In the one with a large brass cap a small wooden disc could be inserted with threads distributed round its edge; when the glass was excited the threads stood out from the edge of the disc. Constructed about A.D. 1720.—(Museum of King George III., King's College, London.)

Daniell's battery, employed in researches by Prof. Daniell.

Early voltaic batteries:—Babington's, Cruikshank's, Wollaston's, and Sturgeon's.

Hare's calorimotor, or deflagrator.

Gas voltaic battery, devised by W. R. Grove, M.A., F.R.S., Professor of Experimental Philosophy in the London Institution, and described by him in a communication read before the Royal Society, May 11th, 1843.

A constant gas voltaic battery, also devised by W. R. Grove, M.A., F.R.S., and described by him in a communication to the Royal Society, dated May 30th, 1845.

Apparatus by which Forbes produced an induction spark from a natural magnet.

"Thunder house," or model to illustrate tbe identity of lightning and electricity, and the use of lightning conductors in protecting buildings,—said to be the first model of the kind, and to have been made by Dr. Priestly.

Apparatus erected in the equatorial room of the Kew

Observatory, in 1843, by Ronalds, for the purpose of observing atmospheric electricity, consisting of a principal conductor, with its glass support, umbrella, and heating apparatus; its voltaic collecting lantern; Volta's electrometers and sights; a Henley electrometer; a Gourgon galvanometer; a discharger, or spark measurer; and a Bennet's gold-leaf electroscope.

Early rheostat, given by Faraday to Sir Charles Wheatstone.

The original Wheatstone's bridge.

Cooke and Wheatstone's earliest needle instrument, 1837. —The letters are indicated by the convergence of two needles. The five line-wires required for the instrument were inserted in grooves in a triangular piece of wood, and wire laid underground.

Original five-needle telegraph dial.—(Wheatstone Collection of Physical Apparatus, King's College, London.)

Three A B C telegraph-sending instruments, showing gradual improvements.

First electric key, constructed by Sir Charles Wheatstone.

Wheatstone's first relay instrument.

Two early forms of stereoscope.—(Wheatstone Collection of Physical Apparatus, King's College, London.)

Jewel lens (ruby) of 1-60th inch focus.—Produced by Mr. Andrew Pritchard, at the suggestion of Sir David Brewster.

Brewster's patent kaleidoscope (with case).—The original form of the instrument made by Bate, of London, in the year 1815.

Otto von Guericke's original air-pump.

Air-pump wlth two barrels.—The first pump of the kind ever constructed. It is an exhausting and a condensing pump, and was made for King George III. in 1761.

Original spirit thermometer, of the Florentine Accademia del Cimento (17th century).—Presented to the Royal Institution by Sir Henry Holland, Bart., F.R.S.

Wedgwood's pyrometer, invented in 1782.—Dry clay when exposed to high temperatures contracts uniformly, and Wedgwood believed that by the amount of contraction the temperature which produced it could be measured. The instrument, however, is not reliable. This specimen was made by Josiah Wedgwood, and presented to the Edinburgh Museum by his grandson, Mr. Godfrey Wedgwood.

Daniell's pyrometer, employed in researches by Professor Daniell.

Astronomy.

Objects illustrating the History of the Telescope and Astronomical Observation.

Incomplete telescope with broken lens of Galileo.
Compass of Galileo.
Magnet of Galileo.
Telescope of Galileo.
Object-glass (broken) of Galileo.
Telescope of Torricelli.
Tube of Torricelli.
Telescope of Diviani.
Telescope of Mariani.
Telescope of Campani.
Telescope by Amici.
Lens by Benedetto Bryhens.
" Primo mobile " of Ignazio Dante.
Quadrant of Cosimo I.
Quadrant of Giusti.
Compass of Antonio Blaichini.
Graphometer of Botti.
Registering thermometer of Fontani.
Natural magnet of the Accademia del Cimento.

Telescope, by Chr. Huyghens. The objective ground and polished by him, and bearing his signature.

Terrestrial refractor, made by Van Deyl, at Amsterdam, in the year 1781.

The Herschel 7-foot telescope. The original instrument constructed by Sir W. Herschel.—The tube is 7 inches in diameter and 7 feet long. Both mirrors were finished by Sir W. Herschel himself; they are sound and whole, but are much tarnished, and the large mirror was damaged in a fire some years since. The framework of the stand is entire, but the moving screws, cords, &c., are useless in their present condition.

A 10-ft. Newtonian reflecting telescope, made by Sir Wm. Herschel in 1812, with $8\frac{1}{2}$-inch large mirror, small plane reflecting mirror, and several eye-pieces of various powers.

Objectives and eye-pieces of the 17th and 18th centuries, the greater part of which were ground and polished by Christian and Constantine Huyghens.

Apparatus used by Baily in repeating the Cavendish experiment.

NOTICES OF BOOKS.

Religion and Science; their Relations to Each Other at the Present Day. By STANLEY T. GIBSON, B.D. London: Longmans, Green, and Co.

IN this work an interesting and delicate subject is handled with a rare amount of breadth, liberality, and candour. The reconciliation of religion and science is attracting much attention, and is being discussed from very various points of view. But may we not put a preliminary question as to the nature of the feud for which a remedy is sought? Are religion and science essentially and permanently hostile? We think not: the differences between them seem to us to spring from merely incidental causes, and to have been intensified by the conduct of injudicious partisans. There are, on the one hand, men of an anti theological turn of mind, who eagerly watch the results of research in the hope of finding some fact or some conclusion which may serve them as a weapon against religion, and who claim—sometimes with very questionable authority—to speak in the name of science. There are men, on the other hand, of decidedly theological leanings, who scrutinise the career of discovery with equal attention, but with an opposite feeling, jealous lest either fact or theory should operate to the prejudice of religion. To take a recent instance, the Darwinian controversy has been needlessly obscured by outsiders of these two classes, the one hoping and the other fearing that the doctrine of Evolution—or at any rate that of Natural Selection—might shake some of the fundamentals of Christianity. More judicious minds, however, could not fail to perceive that Evolution, even when extended to the human race, is no less compatible with theism than was the old notion of special creation. We say that every individual plant and animal is created by God, even though we know it has been procreated by antecedent beings of its own species. In a manner closely analogous, we may still maintain that every animal and vegetable species has been created by God, even though we discover that it has been evolved from some earlier form of organic life. Evolution may be regarded as a mode of creation, not a distinct and irreconcilable process.

Another cause of discord between science and religion may be sought in the circumstance that the sacred writings of most religions open with a cosmogony, and contain passages which may be taken as deliverances on various physical and biological questions. Upon these cosmogonies and these passages theologians still, in some quarters, consider themselves entitled to build up philosophies which are in glaring contradiction to facts.

For this evil the true remedy was pointed out long ago by Giordano Bruno, and afterwards still more clearly by Galileo. These two great reformers maintained that all such passages express merely the opinions common at the times when the Scriptures were written, and were never intended as a physical revelation. We need scarcely say that this judicious view has been rejected by so-called "infidel" writers quite as decidedly as by ecclesiastics. The latter declare that Science and the Bible are at variance, and that science therefore is to be denounced and execrated. The anti-theologians, equally asserting that Science and the Bible differ, infer that the latter is therefore worthless as a moral and spiritual revelation. But religionists of a deeper and wider insight, who can discriminate between the unchanging essence and the varying form of divine truth, and men of science who profess not to be wise above what is demonstrated, see that these discrepancies and contradictions are of little moment. The first step, therefore, in this great reconciliation is of a practical character. Let us not over-rate the functions of the imagination in matters of science. Let us view the sacred cosmogonies as epical versions of the one great truth,—confirmed, as our author shows, by some of the most splendid results of modern discovery,—that "in the beginning God created the heaven and the earth," and let us not recognise, as the authorised and responsible interpreters of Science, men who have comprehended little of her doctrines and less of her methods.

Mr. Gibson's work consists of three essays, treating respectively on the belief in God, on the miraculous evidences of Christianity, and on the relation of the Gospel to the moral faculty in man.

In the first of these essays the author admits that the Design argument, "once so popular" and deemed so conclusive, no longer holds its former position, "at least with the educated classes." Some of the objections which it suggests are here stated with great force. It is admitted that on Paleyan principles, if consistently carried out, the designer and contriver of the universe must himself have been in turn designed, and so on *ad infinitum*. It is further conceded that from effects which, however great, are still finite, we are not justified in assuming an infinite cause. The argument is further met by the answer "there are multitudes of things in the world which do not appear to be the work of infinite wisdom and goodness, or indeed of wisdom and goodness at all." If Paleyans reply that "Were we but wiser we should see wisdom everywhere," they lay themselves open to the retort that "Were we wiser the result might just as likely be the very opposite. Man finds a wise arrangement in the properties which render the sheep, the ox, and the horse subservient to his purposes. But when he reflects on adaptations which render him liable to be devoured by the tiger,

fatally bitten by the cobra, perforated and ulcerated by the chigoe and by *Lucilia humanivora*, he dismisses the matter as inscrutable. It has generally been assumed, in the Design argument, that contrivance must necessarily be the result of self-conscious intelligence. But it is admitted by some that much of man's intellectual action is unconscious. One eminent author goes so far as to maintain that all animals except man are invariably unconscious of their own activity. To the Paleyan, then, declaring that design implies an intelligent designer, it may be replied—"Yes, but who tells you whether this designer is conscious or unconscious?"

By far the most convincing argument for the existence of God is one which has been elaborated by modern science, and which depends on the tendency of all natural forces to come to an equilibrium. Hence we argue that the universe cannot have existed from all eternity, but must have had a beginning in time. Such a beginning, as far as we can conceive, involves the existence and intervention of God. Without such intervention, also, it seems difficult to account for the first appearance of life upon our globe. The experiments made to determine whether spontaneous generation is possible in solutions or decoctions of organic matter,—such as turnips, hay, or cheese,—however ingenious, are all beside the question. Before organic life had prevailed upon our earth, there could be no organic matter in existence. The advocates of abiogenesis, to establish their case, must be able to produce Bacteria, or some other low form of organic life, from inorganic matter.

Great weight is laid by Prof. Clerk Maxwell upon the properties of atoms as a proof of creation. In his Bradford lecture (1873), quoted in the work before us, he declares that "We are unable to ascribe either the existence of the molecules or the identity of their properties to the operation of any of the causes which we call natural." He agrees with Sir John Herschel that "the exact equality of each molecule to all others of the same kind gives it the essential character of a manufactured article, and precludes the idea of its being eternal and self-existent." But if atoms are manufactured articles, where and what is the raw material? The argument of Prof. Clerk Maxwell assumes too much. The bodies called elementary have a merely provisional right to this character; but the decomposition of a supposed element means a division of its molecules. There is, again, one circumstance which seems to point to the disappearance of atoms by a process somewhat analogous to the extinction of organic species. Looking over a table of atomic weights, we find certain gaps in the series, as if links formerly existing had been lost. Nor do we see that the idea of atoms being "eternal and self-existent" is in any way precluded. The author here remarks—"If it be granted that with our present knowledge we can point to no natural agency that would modify or produce the

character of an atom, are we therefore justified in saying that no such agency ever existed? Because we do not know the natural cause of any phenomenon—nay, if you will, cannot conjecture how it could have had a natural cause at all—is it safe at once to pronounce it supernatural?"

Mr. Gibson holds that modern science has, at any rate, the merit of rendering polytheism an impossibility. We are not sure that the compliment can be accepted. We may, indeed, in view of the identity both in matter and in forces which we have succeeded in tracing far beyond the limits of our own planet, admit that the idea of conflicting territorial deities can no longer be entertained. But just as in a State unity of law, of institutions, of language and manners, may prevail equally under a personal despotism, a constitutional monarchy, or a republic, so the homogeneity which we detect in the universe supplies no absolute proof as to whether it is ruled by One or by many.

Into the interesting critique on the "Unseen Universe," and the discussion on that old and vexed question the Origin of Evil, we cannot now enter.

The second essay, on the "Miraculous Evidences of Christianity," can scarcely be dealt with in these pages. But the section on Miracles, so-called, said to have occurred subsequently to the age of the primitive church, and even down to our own times, is profoundly interesting. "There must," says the author, "one would say, be some unknown principle, either psychical or physical, to account for kindred narratives springing up in quarters so remote; and surely we may add the reflection that, with such accounts so attested at the present day among ourselves, it must be hard to establish an exclusive case for the miraculous in Judæa eighteen centuries ago." He adds, however,—"We may also deem it probable that there are ill-understood powers of nature which at times simulate the miraculous."

It is not too much to say that if all divines were like Mr. Gibson the antagonism between religion and science which he endeavours to remove would scarcely have come into existence.

The Variation of Animals and Plants under Domestication. By CHARLES DARWIN, F.R.S. Second Edition, Revised. London: John Murray.

To give a formal critique of this work would be impossible without entering upon the entire debate between the author on the one hand and the anti-evolutionists and catastrophic evolutionists on the other, a task which, for obvious reasons, is here out of the question. The author informs us that since the ap-

pearance of the first edition of the work, in 1868, he has accumulated a large body of additional facts, the more important of which have been made use of to strengthen the argument. Doubtful statements have been omitted, and certain errors discovered by critics have been eliminated. The value of the work has thus been greatly increased. Even those who feel or affect a horror at the very name of the illustrious author cannot, we think, deny that he has laid before the world in this, as in his other books, a store of valuable facts such as has rarely been amassed before. Even if his theories should in the course of time be merged in some wider and more complete generalisation, his merit as an acute and patient investigator of facts can never be disputed. If any man wishes to become a naturalist, we would say to him—"In addition to the actual study of things, read Darwin; read him pen in hand, not necessarily with unhesitating belief, but critically, or if you will even sceptically, so long as your scepticism is candid and scientific. If you do not find everywhere suggestions how, where, and what to observe, you have mistaken your vocation, and will never make a worthy student of animal or vegetable life.

For the benefit of those who possess the first edition of this book, a table of additions and corrections is appended.

Physical Geography, or the Terraqueous Globe and its Phenomena. By W. Desborough Cooley. London: Dulau and Co.

This book begins with a preface in which the nature and scope of "Physical Geography" are discussed. On this subject it would appear that some diversity of opinion, not to say confusion, prevails. The ordinary distinction between Physical and Political Geography is characterised as "a feeble and useless attempt to distinguish between a description of the earth and a description of the countries and kingdoms of the earth, the natural philosophy involved with geographical considerations being in the meantime forgotten."

The most important and interesting portion of the work is the last chapter, in which the author, though not hostile to the nebular hypothesis of Laplace, combats the ordinarily received notion that the earth was at one time a liquid globe. He declares that "in order that it should take a spheroidal figure it was not necessary that it should be fluid." Of an original fused condition there remains on the earth no trace. "Since aërolites," he asks, "are continually falling on the earth, why might they not have fallen in the first instance? Why may not the earth, or at

least the exterior portion of it, have been formed of mineral particles solidified before they met to form a globe ?"

He thinks it "natural to suppose" that the earth was, in the beginning, a perfect spheroid, as symmetrical as if it had been turned in a lathe. He complains that "the most remarkable and eventful crisis in the history of our globe is passed over in silence by the geologist, who begins his history of the earth's formation with uniformity and the established routine of Nature; that is to say, he supposes equilibrium to be established, the conflict of natural forces being at an end and the earth's system complete, and yet affects to start from the beginning."

The alternate rising and sinking of the land, generally admitted by geologists, he denies, or seeks to ascribe it to fluctuations in the level of the sea. That the testimony of the coral islands must not be neglected he admits. This testimony, we suspect, he will have great difficulty in reconciling with his views.

In a section on the effects of lightning we are told that, "at Graaff Reynett, in the eastern division of the Cape Colony, the Court-house was some years ago struck by lightning; and the bell-wire running through the building, and altogether about 80 feet in length, fell to the ground, cut into small pieces of equal length (about three-quarters of an inch)." A precisely similar occurrence has since been witnessed in a workhouse in the North of England. As further freaks of lightning we find it mentioned that 2000 goats, driven for shelter into a cave in Abyssinia, were all killed by a single stroke of lightning. It is also stated that some years ago a French regiment, on the march near Lyons, was completely prostrated by lightning, the men being all thrown down in succession (?), but none killed. These facts will furnish electricians with matter for study. The meteors known as "fire-balls" he considers eminently unaccountable. "Their frequent occurrence and dangerous character are well attested; yet nothing is known with certainty of their nature." That they are rightly to be classed with electricity he doubts. "Compact electricity, hovering slowly in the air, apparently without either attraction or repulsion, rolling on the ground, and then suddenly exploding like a bombshell, is a compound of contradictions hardly conceivable." "It is for chemists to decide," he adds, "whether gaseous matters generating electricity may not by some means be concentrated in the atmosphere, and explode by mixture with oxygen gas at a certain stage of their combustion." Globes of a gaseous mixture rolling on the ground, or slowly hovering in the air and behaving like potassium when thrown upon water, are also not very readily conceivable.

The author's explanation of "glacial epochs" is to some extent that of Mr. James Croll, whose chronology of the cold periods he however rejects, though praising him for "having had the courage to condemn Sir Charles Lyell's doctrine of uniformity, generally received by geologists."

The following speculation is at any rate curious:—"A diminution of the weight of the atmosphere may be justly inferred from the fact that extinct species of animals, as seen in fossil remains, appear to have been collectively created on a larger scale than the corresponding species at present existing. Heavy animals were relatively more numerous, and weight rather than agility characterised the figures of all." Now, that certain extinct animals must have been larger than any now existing is admitted; but we are unable to discover any regular and serial decrease from the earliest epochs downwards. "Under a very heavy atmosphere," says Mr. Cooley, "the elephant would feel relieved of much of its weight, and become a comparatively active animal." Unfortunately the elephant and the rhinoceros, clumsy as they look, are decidedly active animals.

As a theorist we do not think the author happy; but he has a remarkable facility for collecting anomalies and pointing out unsolved problems. Hence this work may be pronounced exceedingly suggestive, and must be recommended to the careful study of geologists.

Practical Plane Geometry. By E. S. Burchett. London and Glasgow: W. Collins, Sons, and Co.

This work is in its objects and nature totally different from "Euclid's Elements." It has been compiled, as the author informs us, "on account of a strongly-felt want of a more complete text-book upon the subject." Its aim is practical, not theoretical. Hence, beyond the introduction, it consists of problems and of applied geometry, including such subjects as the repetition of geometric figures to fill plane surfaces, the lines of arches, the curves of mouldings, Gothic tracery, and the construction of scales. An Appendix treats of the elements of orthographical projection.

We consider the work likely to prove of no small value to architects, builders, and to designers of ornamental work in metals, glass, and other materials.

Annual Report of the Board of Regents of the Smithsonian Institution for the Year 1874. Washington: Government Printing Office.

This yearly issue contains the Report of the Secretary, Prof. Henry, including the financial statement; list of publications issued and in the press; account of researches conducted in

meteorology, astronomy, physics, natural history, and ethnology; account of international exchanges, &c. The National Museum of the United States appears to have received many and valuable additions from almost all parts of the world.

The accompanying literary matter consists of the Eulogy of Laplace, by Arago, as delivered before the French Academy; the Eulogy on Quetelet, by Ed. Mailly; and of A. A. de la Rive, by M. Dumas. It may be interesting to our readers to learn that, in addition to his well-known eminence as a mathematician and statistician, Quetelet possessed considerable merit as a poet—a somewhat rare combination of talents. There are also papers on "Tides and Tidal Action in Harbours," by Prof. J. E. Hilgard; "Observations on the Electricity of the Atmosphere and the Aurora," by Prof. Lemström; on a "Dominant Language for Science," by Prof. de la Carpolle; on "Underground Temperature," by Messrs. Schott and Everett; "Earthquakes in North Carolina," by Prof. Warren du Pre; "Transactions of the Society of Physics and Natural History of Geneva, June, 1872, to June, 1873;" a "Report on Warming and Ventilation," by A. Morin; and a number of brief ethnological notices.

Some Account, Critical, Descriptive, and Historical, of Zapus Hudsonius; *and on the Breeding-Habits, Nest, and Eggs of the White-tailed Ptarmigan,* Lagopus leucurus. By Dr. Elliott Coues, U.S.A. Washington: Government Printing Office.

A couple of useful monographs, which the binder has unfortunately blended together in a curiously perplexing manner. It is singular that the *Zapus Hudsonius* has been honoured with no fewer than forty-four synonyms, and has been referred to the genera *Dipus*, *Gerbillus*, *Meriones*, *Jaculus*, and *Mus*.

An Account of the Various Publications relating to the Travels of Lewis and Clarke, with a Commentary on the Zoological Results of their Expedition. By Dr. Elliott Coues, U.S.A. Washington: Government Printing Office.

This pamphlet, extracted from the "Bulletin of the Geological and Geographical Survey of the United States," consists of two portions, the bibliographical and the zoological. The author, having frequent occasion to consult the work for facts bearing upon the zoology of Western North America, found quotation

impossible without explicit reference to some especial edition. Neither of these early explorers became the actual author of an account of their joint expedition and its results; but whilst their notes were being edited two separate and incomplete versions fell into the hands of publishers, and have become the groundwork of a number of apocryphal editions.

The zoological portion consists of a catalogue of the mammalia and birds discovered or observed by Lewis and Clarke, with references for each to the respective editions.

The Cholera Epidemic of 1873 *in the United States.* By J. M. Woodworth, M.D. Washington: Government Printing Office.

Under this title we have a volume extending to upwards of a thousand pages, which, though ostensibly designed merely as an official report on the last epidemic of cholera in the United States, presents a great amount of valuable information on the sources, propagation, and treatment of this dreaded pestilence. The author does not accept the theory that cholera is transmitted in the atmosphere, or depends upon occult cosmic influences. From the facts which have come under his observation he deduces the following propositions:—

"I. Malignant cholera is caused by the access of a specific organic poison to the alimentary canal, which poison is developed spontaneously only in certain parts of India.

"II. This poison is contained primarily, so far as the world outside of Hindostan is concerned, in the ejections—vomit, stools, and urine—of a person already infected with the disease.

"III. To set up anew the action of the poison, a certain period of incubation, with the presence of alkaline moisture, is required, which period is completed within one to three days; a temperature favouring decomposition and moisture or fluid of decided alkaline reaction hastening the process, the reverse retarding.

"IV. The period of morbific activity of the poison is characterised by the presence of bacteria, which appear at the end of the period of incubation, and disappear at the end of the period of morbific activity; that is to say, a cholera ejection or material containing such is harmless both before the appearance and after the disappearance of bacteria, but is actively poisonous during their presence.

"*Note.*—It is not meant by this that the bacteria so found are the cholera-poison, since they differ in no appreciable manner from bacteria found in a variety of other fluids. Indeed Lebert hints that the bacteria may even be the destroyers of the poison."

In another passage, however, we are told that Dr. Nedsvetzky, of Yaroslav, near Moscow, "has apparently discovered the cholera bacterium, which is developed in enormous quantities in the discharges. He found that quinine, camphor, carbolic acid, tar, calomel, and chloral had no effect upon them; that opium, nux vomica, and chloroform killed them slowly; while tannin, sulphate of iron, chlorine water, and dilute sulphuric, nitric, and muriatic acids killed them rapidly." He suggests the latter six remedies as the most efficient against cholera.

The author also considers that the cholera-poison may be destroyed by acids, and recommends them—especially dilute sulphuric acid—as prophylactics. The value of "sulphuric acid lemonade" is indeed fully shown by the account given by Dr. Curtin of his experience at the Philadelphia Hospital in 1866. During the time the acid was used only one new case occurred. "This was a poor lunatic, who, upon tasting it, spat it out, and surprised me very much by saying with great vehemence—'Docther, you call this limonade, but ye can't desave me; it's nothing but ile of vitriol.'" This woman took the disease, and died. Districts where the air is saturated with sulphurous fumes have been remarkably free from cholera, and persons whose employment exposes them to similar vapours have enjoyed a striking immunity. We may here remark that not a few independent observers have formed a very high opinion of the value of sulphuric lemonade as a beverage in hot and malarious districts, and consider it decidedly superior to the vegetable acids.

Unlike many diseases, cholera shows little marked preference for age, sex, race, or condition of life. The negro race seem to suffer more than Europeans. "The disease was most malignant among the lower orders of each community, but the better classes were by no means exempt."

The author's strictures on the sanitary condition of many cities in different parts of the world, though severe, are only too well founded. Much as still remains to be done before the sanitary condition of England is satisfactory, it is some consolation to know that we are in advance of our Continental neighbours.

An interesting and valuable feature in this work is the bibliography of cholera, extending to over three hundred pages.

Report of the United States Geological Survey of the Territories. By F. V. Hayden, United States Geologist-in-Charge. Vol. II. Washington: Government Printing Office.

This volume, containing an account of the Vertebrata of the Cretaceous Formations of the West, is a document absolutely priceless to the palæontologist, and reflects the highest credit on

its authors, and not less on the Government which so liberally places it in the hands of learned societies throughout the world. In addition to illustrations inserted in the text, there are fifty-three large lithographed plates, most carefully executed and well explained. Such work as this volume represents is absolutely necessary for the progress both of geology and zoology. At the same time it is evidently beyond the means of private individuals. We can only therefore wish that the noble example set by the Government of the United States may be followed elsewhere. Incidentally we may notice that the measurements of the various fossil remains investigated are given on the metric system.

The authors infer from their researches that the distinct faunal areas which the earth now presents—the more prominent among which are those respectively of Australia, of South America, and of the temperate regions of the northern hemisphere—have always prevailed, so far, at least, as the extinct Mammalia are concerned. The writers ask—"Was the succession of interruptions of life universal over the globe, and do these trenchant lines justify the old assumption of repeated destructions and recreations of animal life? The former question has already been answered in the negative by the explanation of the characters of the existing faunæ of the southern hemisphere, where ancient types still remain in considerable numbers. Moreover, some of the later periods, both of North America and Europe, are characterised by a large predominance of forms of the corresponding southern continent. It is, indeed, evident that migration from the one continent to the other has taken place, and is amply sufficient to account for the abrupt changes in the life of each without necessitating the intervention of creative acts. If glacial periods be dependent on cosmic movements, the increased obliquity of the earth's axis to the sun at periods 25,000 years apart would cause a corresponding alternation of cold periods in the opposite hemispheres. This is well known as a most potent cause of migration and extinction, and the known relations of the faunæ would thus result from a greater or less alternate invasion of one hemisphere by the life of the other."

The distinct terrestrial faunæ within these great periods are referred to the "alternate presence and absence of water-areas adapted for the preservation of animal remains,"—a variation due to the slow vertical oscillations of the earth's crust.

Hence it appears that the authors do not find any evidence favourable to the vicious principle of *omnia per saltum*,—a reactionary doctrine whose rehabilitation is in some quarters so earnestly attempted.

Tables, Nautical and Mathematical, for the Use of Students, Seamen, Mathematicians, &c. Arranged, Corrected, and some Re-calculated. By Henry Evers, LL.D. London and Glasgow: W. Collins and Sons.

The author remarks, in his preface, that his object has been "to arrange a new set of handy, clear, and correct mathematical and nautical tables, for the use of the student and expert practitioner in mathematics, navigation, nautical astronomy, steam, surveying, &c." This task appears to have been satisfactorily fulfilled: the tables are well selected and clearly printed.

Geology of Otago. By F. W. Hutton and G. H. F. Ulrich. (Printed by order of the Provincial Council of Otago.) Dunedin: Mills, Dick, and Co.

It is pleasant to find that in the "Britain of the South" Science is not neglected. The volume before us gives an account of the physical geography, the geology, descriptive, historical, and economic, of the province of Otago. The mineral wealth of the district appears to be considerable. Antimony occurs in considerable quantity, as stibnite, at the Arrow River, Carrick Ranges, Waipori, and other places. Tungstate of lime occurs plentifully in various parts. Alum shales are found at Waikouaiti, but their practical value is doubtful. Retinite is so plentiful in the lignites that it is collected for the manufacture of varnish. There is a lode of copper at Moke Creek, Lake Wakatipu, but no other has been discovered. Hæmatite containing 94 to 96 per cent of oxide of iron occurs in a lode 6 feet thick at Moori Point, on the Shotover, and at Port Molyneux, and good clay ironstone exists at Tokomairiro. As in all these places lime and coal can be obtained at no great distance, iron-works will doubtless spring up. Titaniferous iron-sand is found in quantity at Port William, in Stewart Island. Iron pyrites, an important element in the industrial development of a country, are said to be common, but we can find no mention of the percentage of sulphur, or of the presence or absence of arsenic. Silver, platinum, cinnabar, and native mercury are mentioned in the list of minerals without full information as to their quantity. Wavellite occurs, but no mineral phosphate of lime appears to have been discovered. The coal-fields are important—that of Tokomairiro is estimated to contain 768,000,000 tons of available coal, after making the usual deduction of one-third for waste, &c. Thus New Zealand can enjoy, on the one hand, the advantages of manufactures, and on the other the nuisances of smoke, of colliers, and perhaps at some future date of a "coal-ring."

Gold is, however, at present the mineral commodity most sought for in Otago. The following method is given for an approximate determination of gold in pyrites:—"Weigh a good average sample of the dry ore—say about 2 lbs.—and roast it till perfectly *sweet* (on a shovel over the fire will do), *i.e.*, till no more smell of sulphurous or arsenious acid is perceived on stirring. Place the roasted mass in an iron mortar, mix it with so much water that it just forms a *very stiff* paste, and add a table-spoonful of quicksilver. Rub with the pestle till all the quicksilver has disappeared. A second similar amount of quicksilver is then worked through in the same way, and then hot water, a little soda, and about five or six table-spoonfuls of mercury are added, and the mass gently stirred for some time, to allow the finer particles of mercury to settle down and unite with the large lot at the bottom just put in. Now follows the careful washing away of the red oxide of iron slime, in an enamelled iron dish, and ultimately the retorting—at not too strong heat—of the whole of the mercury collected. From the weight of the gold left behind the gold per ton can be easily calculated. The experiment closely imitates the process adopted on the large scale, and, if carefully executed, gives within 80 to over 90 per cent of the fire assay."

In an Appendix there is an account—accompanied with diagrams, drawn to scale—of the German metallurgical furnace for burning brown coal, known as the "Treppen-rost."

In speaking of the soils of the province, which are characterised as being above the average in quality, and which if carefully managed will prove a source of wealth after the gold-fields are worked out, the authors make a very significant remark:—"The gold-miners are in league with the rivers, and we may feel sure that before many years are passed considerable quantities of agricultural land will be either washed away or covered with 'tailings.'" This is one of the many cases, not dreamt of in the philosophy of the Manchester school, which prove that individual covetousness—or, if you will, individual industry—does not, when left to its own guidance, invariably promote the general good.

Turning from economic to historical geology, we find it mentioned that in 1874 Mr. W. L. Travers brought forward conclusive proof that New Zealand has never been covered by an ice-sheet, or, in other words, has never experienced a glacial epoch. Mr. Hutton, from his own observations, also, maintains that neither in the pliocene nor in the pleistocene times has New Zealand experienced a colder climate than at present. This conclusion, if substantiated, is of great importance.

The fauna of the province is remarkable for its poverty. Sixty-one species of land-birds in a country not over-peopled, and infested neither with Whitechapel bird-catchers nor with French sportsmen, is a very meagre list. Of these, six species belong

to the parrot group. The insects, curiously enough, are not mentioned: this is a very serious omission, not merely from the point of view of the scientific naturalist, but from that also of the farmer. The flora of the province has also been overlooked. Wheat is extensively grown in the lake-districts at from 1000 to 2000 feet above the sea-level, but the remark is added that this is not to be taken as the limit of its successful cultivation: this indicates an agricultural climate very much superior to that of England. The forest-line is given at 3400 and the snow-line at 7500 to 8000 feet. We find, also, some interesting remarks on the character of the scenery, which is declared to be eminently beautiful. The mountains, which reach an altitude of nearly 10,000 feet, are exceedingly bold in their outline. "The lakes present scenery unsurpassed probably in the world, for, unlike the Swiss lakes, they do not lie outside the principal mountain masses, but wind close round their feet." Waterfalls are few and small.

The work is illustrated with several plates, showing sections, &c., and with a geological map on the scale of 24 miles to the inch.

Practical Physiology. By Edwin Lankester, M.D., F.R.S., Sixth Edition. London: Hardwicke and Bogue.

This work was originally entitled "School Manual of Health," and was intended to serve as a reading-book in primary educational establishments. The author, however, finding that it was principally used "as a text-book in classes formed for the study of physiology in its relations to health and life," has changed its title, and added a large number of illustrations, a series of questions, and a glossary. The result is a book containing an abundance of sound and valuable practical information, mixed, however, with speculative passages of an occasionally questionable nature. In the Introduction the author enters upon that vexed question, the scope and the character of mental training suitable for various classes of the community. Here we are struck with the following dictum:—"I wish, however, to state my conviction as a physiologist that there is no anatomical distinction between the brains of rich and poor people!" We have rarely seen a truism—admitted everywhere, save, perhaps, in the Manchester Chamber of Commerce—enunciated with greater solemnity. But this undoubted truth has, after all, little direct bearing on the subject. The question is not to what degree of education persons of such and such a class are entitled, but what is practically attainable. So long as the majority of youths have to enter upon their business at an age below, say, eighteen, so long must a broad and thorough mental culture be not the

rule, but the exception. Seldom has a deadlier blow been struck at the well-being of our race—physical, intellectual, and moral—than when young children were first led or driven into an industrial career. One no less deadly is aimed by those who, under what pretext soever, aim at transferring women from the household to the workshop and the office.

Dr. Lankester's remarks on the absence of physiological knowledge among statesmen, municipal bodies, and literary men are excellent. "To those," he writes, "whose professional avocations lead them to speak on physiological subjects, it is often a source of great annoyance to find that their remarks have been thoroughly misunderstood by the ignorance of physiology among those whose duty it is to supply information through the press." But this complaint admits of generalisation. The ignorance of physiology is merely part and parcel of that want of scientific culture of which we daily meet with strange instances among the "educated and respectable classes." Not long ago, in an eminent daily paper, we came upon a lengthy notice of an astronomical work, in which the reviewer gravely stated that all gases would explode at temperatures far below redness, the product of the explosion being "a vacuum, with a few grains of dust." This reminded us of a fellow-student of ours, long ago, who on being required to analyse a mineral gave in a certain percentage as "dirt."

But ignorance is far from being the only reason why town-councils and vestries meet sanitary regulations with cold support, if not even with passive opposition. Disease and dirt are vested interests, and are generally influentially represented in town-halls and board-rooms.

Dr. Lankester evidently holds that a more generally diffused acquaintance with physiology would be a great boon to the community. He says—"The sum of human suffering that might be prevented and the amount of wealth that might be saved by a knowledge of the laws of disease is incalculable." And again —"As long as this subject is thus slighted and neglected in our plans and systems of education, so long will the miseries that arise from premature disease and death occur, and so long will poverty and physical debility obstruct the progress of mankind in the path to wealth and happiness." Now no one can deny that many persons are reduced to want, and may even become burdens to the public, in consequence of disease. It must further be conceded that if a child dies before arriving at the age when productive labour becomes possible, all that has been expended on its nurture and education is wasted. But, on the other hand, if every person born were to survive to the age of a century and upwards, as in the model city Hygeia, or even to the traditional three score years and ten, unless the means of subsistence could be increased in a corresponding proportion the struggle for existence would be fearfully intensified, and destitution would

prevail to an extent which is now utterly inconceivable. Thus, in their efforts to remove one evil, philanthropists, if they succeed at all, are apt—like the village tinker—to create a greater.

Turning from this interesting and suggestive Introduction to the body of the work :—

The remarks on drinking-water call for notice. "Hardness" may be in part caused by salts of magnesia, as well as lime, and in the former case relatively small quantities are objectionable. We should not place much confidence in the benefits to be obtained by boiling water containing organic impurities. Certain volatile matters would doubtless be driven off, and "germs," if present, might be destroyed, but there would still be an offensive and dangerous residue. Nor can we approve of the use of condemned waters for washing. A test for water, still simpler than the addition of Condy's fluid, is to cork up a quantity of it in a clean bottle, and leave it for a few hours in a rather warm place. If, on uncorking the bottle, a bad smell is perceived the water should be rejected.

Dr. Lankester very judiciously condemns cold meat, that haunting sin of the English *cuisine*. He says—"When cold food is taken it reduces the temperature of the stomach, and both the nerves and vessels of the stomach are taxed in order to bring the temperature of the food thus taken up to that of the human body. It is only in very hot weather or in tropical climates that food can be taken with advantage when cold." We fear, however, that this authoritative condemnation of cold mutton will not meet with the approbation of his lady readers.

On the subject of clothing Dr. Lankester gives very good advice. He declares that "black and dark substances, whilst they absorb heat best, also radiate or give it off quickest, so that it is really better to wear light-coloured clothes both in summer and winter. The true reason why the civilised inhabitants of Europe and America dress in dark-coloured clothes both in summer and winter is economy. It is a question of *soap* and *washing*, and not of the comfort or use of the dress." Jean Paul Richter, if we remember rightly, puts this point more tersely. "The ancient Spartans," he says, "wore red to hide blood ; the modern Italians wear black to hide fleas."

The author seems disposed to uphold the old notion of a contrast, or difference of kind, rather than of degree, between man and the rest of the animal world. For this purpose he sometimes goes out of his way. Thus in one place he takes occasion to observe that "the human skeleton has two hands and two feet, and is not four-handed, as in the highest monkeys." But according to the best and most recent authorities the hinder limbs of the highest apes terminate in true feet, and, in accordance with this conclusion, theorists most eager to uphold the "great gulf" between man and beast have abandoned this ground. We regret to see, in a work which has such just

claims to a wide circulation as the present, a classification of the animal kingdom in which the Cuvierian order of "Bimana" is still recognised, whilst the following order, "Quadrumana," is made to embrace forms differing from each other structurally much more widely than do some of them from man. Were these passages modified in accordance with the teachings of philosophic zoology, and were certain doubtful passages in the Introduction withdrawn, we should think ourselves justified in speaking of this book in terms of almost unqualified approbation.

Handbook of Astronomy. By DIONYSIUS LARDNER. Fourth Edition, Revised and Edited by E. DUNKIN, F.R.A.S., of the Royal Observatory, Greenwich, and Secretary of the Royal Astronomical Society. London: Lockwood and Co.

WE once heard tell of an amateur gas-manager in a northern town who could not make good gas, but who accomplished a feat far more difficult. He prevailed upon his townsmen to burn bad gas, to pay for it at a high price, and to believe most devoutly in its excellence. Something similar occurs in the scientific world. We meet from time to time with men who do not shine in original research. They neither reveal to us new phenomena, nor do they make any important step in theory; but they contrive to outshine and almost eclipse the modest scientific worker. They become members—and leading members—of learned societies. Professorial chairs, commissionerships, decorations fall to their lot, and it is generally not till their death that the plain question "But what has he really done?" is frankly asked, and receives no answer. Of such men the Rev. Dionysius Lardner was at one time the type. He was LL.D, F.R.S. L. and E., M.R.I.A., F.R.A.S., F.L.S., F.Z.S., Hon. F.C.P.S., not to speak of minor honours. He was Professor of Natural Philosophy and Astronomy at University College, but in popular belief he was supposed to be a transcendant authority in every department of Science. True, when he went to towns in the North and lectured to mechanics on the steam-engine, some of his audience were rude enough to remark that if they knew no more about the steam-engine than did the *petit-maitre* on the platform they would soon have to "shut up shop." But these critical utterances were lost in the general applause, and it was not till the Professor's moral fall that his claims as a man of science were formally re-considered.

Among the many works which he wrote or edited, "with the assistance of men eminent in certain departments," was the treatise before us.

It must not be inferred from these remarks that we are dissatisfied with its character. On its first appearance it came to

be regarded as an ably compiled summary of the astronomical knowledge of the day, drawn up in "ordinary and popular language," and divested, as far as the subject admits, of puzzling technicalities, and especially of that parade of mathematics which, above all things, deters the general reader. The present editor, working on the same general lines, has embodied in the book the results of more recent research. It is no empty boast when he declares in the Preface that "to the student of the higher or mathematical branches of astronomy this work, however, will also be found interesting and instructive, as he will find information of the most valuable kind in it, for much of which he may look in vain in works of higher pretensions." Beginning with an account of astronomical methods of investigation and means of observation, the author passes to a description of astronomical instruments and their mode of use. It will be interesting for the general reader to compare the gigantic and exquisitely finished instruments described and figured in this section with the telescopes once used by the fathers of astronomical science, now on view in the Loan Collection of Scientific Apparatus at South Kensington.

The next chapters treat of the general rotundity and dimensions of the earth, of its spheroidal form, mass, and density; of the apparent form and motion of the firmament; of the earth's diurnal and annual rotation; of atmospheric refraction and parallax; of precession and mutation. The author next gives a description of the moon, noticing her effect upon the tides and trade-winds; of the sun, and of the solar system in general. Next follows an account of the planets classified under three groups—the terrestrial, the planetoid, and the major or exterior. Eclipses, transits, and occultations are next explained. Thence we are led to a survey of the comets, and of the so-called fixed stars,—a department of astronomy which has been wonderfully enriched during the third quarter of the present century. The work is well illustrated, and is provided with a good index.

We can do no other than pronounce this work a most valuable manual of astronomy, and we strongly recommend it to all who wish to acquire a general—but at the same time correct—acquaintance with this sublime science.

A Critical Examination of some of the Principal Arguments for and against Darwinism. By James Maclaren, Barrister-at-Law. London: E. Bumpus.

It is a trite saying that there are three tasks which every man thinks it within his power to accomplish—to drive a gig, to manage a farm, and to edit a newspaper. To these the experience

of the last few years warrants us in adding a fourth. Every man of culture—no matter how little qualified by especial training or previous study—deems himself entitled to pass an authoritative judgment on the most abstruse questions in organic science. Lawyers, divines, mathematicians, literary men, financiers who have never devoted an hour to the serious study of zoology or botany,—who have never observed a novel fact or verified an old one, who have never even determined a species, and who would be utterly at sea were they to make the attempt,—consider themselves competent to discuss the origin of species, not merely in private, but for the edification and guidance of the public. This is not the case in other departments of knowledge. Some years ago there arose a dispute among physicists concerning two rival views on the nature of light,—the undulatory and the emission hypothesis. Considerable animosity prevailed between the combatants, and some of the language used was decidedly unparliamentary; yet the outside public wisely held aloof, and left the matter to be decided by experts,—men versed in physics and mathematics, and capable of rightly appreciating the evidence adduced on either side.

More recently we have witnessed a discussion in chemistry between the respective partizans of the new and of the old notation. The controversy opened out for many men a royal road to eminence, by giving them an opportunity for writing books for which no *raison d'etre* would otherwise have existed; but the public and the general press did not interfere on the one side or the other. Why it should have adopted so different a course with respect to the origin of species is not clear; it cannot be because the subject is more within the grasp of an unprepared mind. We will venture to declare that zoology and botany, if taken up as sciences, are far more complicated and difficult than chemistry or physics; it may be because the origin of species, that of man included, is a more interesting subject than a theory of light: this, however, is the very reason why it should have been let alone. It is only too interesting, and excites too much passion and prejudice to be dealt with by any but specially disciplined minds.

Turning from the actual to the hypothetical, let us suppose—a thing not entirely inconceivable—that a difference of opinion were to prevail among the legal profession upon some important topic, and that an outsider—a botanist, or a geologist, or an electrician, ignorant of law and relying merely upon his general mental cultivation—should write a book, formally summing up the arguments on either side, and pronouncing a decision, would not all the Inns of Court ring with contemptuous and most justifiable laughter? But where is the essential difference between such a supposed case and the one before us? We know that exception will be taken to this view. Our author remarks that the evidence for or against Darwinism ought to be intelligible to

every educated man. We must confess that we see no such obligation. The evidence upon which a scientific theory rests should be intelligible to a man who has made the science in question his study, just as the specification of a patent must be intelligible to men accustomed to the particular trade or manufacture with which such patent is concerned; but to demand more is unreasonable. It has also been urged that the procedures of logic, like the rules of arithmetic, are equally applicable to all subjects irrespective of their peculiar nature, and that a lawyer accustomed to take no point for granted, to require a reason for everything, and to scrutinise chains of argument, is the very man to deal profitably with such a question as Darwinism.

All these pleas involve half-truths, or, in other words, errors of the most dangerous class. The accountant's sum-total, if obtained according to arithmetical rule, is doubtless true as far as the figures on his paper are concerned; but before we can grant their value we must ascertain whether those figures relate to realities or to mere assumptions. The logician's conclusion, rightly drawn from his premises, will be formally true; but before we can accept it as of objective value we must verify his premises,—and this is precisely what no outsider can do.

We may further submit that there is no necessity for a man to form an opinion at all concerning the origin of species. If he has not the time, the opportunity, and the will to make the question the subject of his especial studies, and if he has little or no previous knowledge bearing on the point, his legitimate course is to suspend judgment. Least of all should he come forward as a teacher before he has been a learner.

We may call the attention of the lawyer who thinks himself qualified to discuss this question to an illustration taken from his own profession. What would be thought of a judge who should pronounce an award after having listened to witnesses speaking a foreign tongue, and whose testimony was interpreted, if at all, only by partisans? We believe we could lay before Mr. Maclaren evidence on the origin of species concerning which he would be unable to tell whether it was favourable or adverse to the views of Messrs. Darwin and Wallace. If we translated it for him into words, he would still have to decide in how far our interpretation was fair or in how far vitiated by party spirit. Nay, even the evidence which is given in books—no matter how ably, clearly, and candidly written—conveys to the working naturalist a very different meaning from what it does to the outsider. The latter has to deal, as it were, not with the direct light, but with that which has passed through a refracting medium.

We are highly gratified with the increased attention paid on all sides to Natural History, due, in no small degree, to the impulse given by Mr. Darwin and his immediate followers; but we would wish all neophytes to master the alphabet of the science before they proceed to grapple with its greatest difficulties. We

must therefore earnestly deprecate the appearance of works like the one before us, believing that they retard instead of promote the definite solution of the question they attempt to discuss.

We must go still further: we utterly repudiate the claims of metaphysicians and philologians, *as such*, to be heard on the origin of species. Prof. Max Müller has possibly traced human language back further than any other investigator, and has made words tell us historial secrets almost as interesting as those which the geologist elicits from the examination of fossils; but words can evidently bear no witness as to a state of things antecedent to their origin. When Prof. Müller declares on the Darwinian theory he is very much in the position of a topographer who, having traced every affluent of the Thames to its source, and, if you will, gauged its flow and analysed its water, should therefore think himself competent to decide on the origin of the clouds and mists by which those affluents were originally fed.

Let us now examine in what manner the author has executed his task. The first point that strikes us is that he does not distinguish with sufficient clearness between *Evolutionism*—the doctrine of a progressive mutation of species—and *Darwinism*,—the explanation of such changes by the hypothesis of Natural and Sexual Selection. The former, we need scarcely remind our readers, may be and is admitted by those who cannot accept the explanation offered by Mr. Darwin.

We next notice that Mr. Maclaren places the views of mere literary men of the world, such as the late Bishop Wilberforce in the same rank with those of original observers, such as Messrs. Darwin and Wallace on the one hand and Mr. Mivart on the other. Are we to infer that he would, as a lawyer, accept hearsay evidence as at all approaching in weight to the testimony of an eye-witness? The criticisms of the Bishop, as taken from his article in the "Quarterly Review" may be interesting as a specimen of the manner in which unscientific minds are apt to deal with scientific questions, but as a contribution to biology they merely provoke a smile. Mr. Maclaren re-quotes from Dr. Wilberforce the passage in which Prof. Owen urges —"How unerringly and plainly the extremest varieties of the dog kind recognise their own specific relationship; how differently does the giant Newfoundland behave towards the dwarf pug, on a casual *rencontre*, from the way in which either of them would treat a jackal, a wolf, or a fox." Yet we have the strongest possible evidence that the domestic dog, *Canis familiaris*, is an artificial product formed by the intermixture of different wild species. Even now the breed is known to be fruitfully and purposely crossed with the wolf and the fox, whilst no one has been able to point out the dog as existing anywhere in a state of original wildness. There is generally more or less manifestation of hostility when a tame animal meets a wild member of its own species. Need we then wonder if a dog meeting a fox or jackal

should show but small tokens of friendship? It is commonly supposed that the entire human race belongs to one and the same species, yet the mutual repulsion between certain of the different strains of man is as well-pronounced as that between the dog and the fox.

We may next notice certain passages extracted from a paper in the ninety-second number of the "North British Review," in which the doctrine of Natural Selection is attacked from what may be called a mathematical point of view. We quote the passage:—"The advantage gained by one individual who has been favourably modified, is utterly overbalanced by numerical inferiority. A million creatures are born, ten thousand survive to produce offspring; one of the million (from a favourable variation) has twice as good a chance as any other of surviving, but the chances are fifty to one against the gifted individual being one of the hundred survivors. No doubt the chances are twice as great against any other individual, but this does not prevent them from being enormously in favour of some average individual. All that can be said is that the favoured 'sport' would be preserved once in fifty times. In the second place, let us consider what would be its influence on the main stock when preserved. It will breed and have a progeny of, say one hundred; now this progeny will be, on the whole, intermediate between the average individual and the sport. The odds in favour of one of this generation will be, say, one-and-a-half to one as compared with the average individual. The odds in their favour will therefore be less than that of their parents, but owing to their greater number, the chances are that about one-and-a-half of them would survive. Unless these breed together, a most improbable event, their progeny would again approach the average individual, and so on until all trace of the original improvement disappeared."

"Suppose a white man to have been wrecked on an island inhabited by negroes, and to have established himself in friendly relations with a powerful tribe whose customs he has learnt; grant him every advantage which we can conceive a white can have over a native, yet it does not follow that after an unlimited number of generations the inhabitants of the island will be white. Our shipwrecked hero might become king, he might kill a great many blacks in the struggle for existence, he could have a great many wives and children. In the first generation there will be dozens of young mulattos much superior in average intelligence to the negroes. We might expect the throne to be occupied for some generations by a more or less yellow king, but can anyone believe that the whole island can gradually acquire a black or even a yellow population?"

This extract is a very good instance of the quietly adroit manner in which mathematicians are apt to beg the question, and we regret to see that Mr. Maclaren has but a very remote

suspicion of the fallacies involved in the argument and the illustration. It is assumed that the outward circumstances remain the same; that young animals are exactly intermediate between their parents, and, above all, that the advantage of the improved "sport" over the average individual is as two to one. Now let us, in turn, suppose that an animal is produced swifter than the rest of its species; that an enemy makes its appearance swifter than the average individual, but slower than the "sport." The chance of the latter surviving and leaving progeny would then not be double that of the ordinary individual, but greater beyond all comparison. Its mate would probably not be one of the slowest of the species, since all such would stand the greatest chance of being devoured by the enemy. Amongst its offspring, judging from facts daily observed, there would be some as swift as itself, and possibly even swifter, and these would become the progenitors of the new race. Or let us take the "North British Reviewer's" illustration of a white man shipwrecked upon an island inhabited by blacks, and there intermarrying with a native woman. Among his children, despite the doctrine of averages which bids a poor wretch shivering over an empty fire-grate on a cold winter's night to feel comfort in the thoughts of a great conflagration at the other end of the street—some will be found very closely resembling their father. If now a new disease fatal to blacks and comparatively harmless to whites, like the measles at Fiji, appears and becomes endemic in the island, the children of "our shipwrecked hero" would escape its ravages just in proportion as they inherited their father's constitution, and the population of the island might in such a case become permanently a light yellow.

On the fertility of hybrids Mr. Maclaren remarks:—"Both Lamarck and our author are obliged to allow the general existence of a great degree of sterility among hybrids, and though they may give some instances of partial fertility, we are strongly reminded of the old canon, exception proves the rule." This "old canon" is in the eyes of men of science about as acceptable as the saying of Sir Thomas Browne—"I believe, because it is impossible." Suppose it were found that the law of definite proportions did not extend to all the elementary bodies, would any chemist contend that such an exception "proved the rule?" So far from it, he would cease to regard the law above-mentioned as a "rule" at all, and consider it merely as a provisional and imperfect generalisation. Leaving on one side the case of the *léporides*, the cross between the hare and the rabbit (whose existence Mr. Maclaren does not think proper to admit), there are cases quite sufficient of fertile hybrids amongst the respective groups of finches, of grouse, and of ducks. So that, though as Prof. Oscar Schmidt happily says, "by ill-luck" the most ancient and common case of hybridisa-

tion—that of the mule and the hinny—supports the common doctrine, all that we can truthfully say is that some hybrids are fertile, whilst others are not, which is simply no rule at all. This subject is one which stands in need of extended and careful experimental research. Only we fear that some one of the "societies" which have taken the morals of the English public under their especial care will some day suddenly awake to the conclusion that such experiments are "immoral, degrading, and deceptive," and will frighten a weak-minded legislature into passing an "Anti-hybridisation Act" for the further restraint of scientific curiosity.

As may well be imagined, our author has found in the course of his reading many difficulties which lie in the way of a full acceptance of the Darwinian theory. Nor is he much better satisfied with rival hypotheses. He pronounces that of Prof. Owen very vague, and that of Prof. Mivart "not very intelligible"—a perfectly just conclusion. The Duke of Argyle, he holds, leads us "at once out of the realm of science into that of miracle."

The fact remains that the question of the origin of species is yet very far from solution. Most working naturalists believe in evolution, and phenomena which scarcely admit of any other explanation, meet us on every hand. But as Sir C. Lyell and Mr. Darwin grant, of the laws of such evolution we are still profoundly ignorant. Yet these we must endeavour to ascertain before we can with any confidence hope to discover how it is brought about. Surely this task may be best accomplished by applying such theories as that of Darwin to the phenomena we observe, noting what they explain and where they fail, and modifying our hypotheses accordingly. Here, as might be expected, Mr. Maclaren takes leave of his readers. After collecting the evidence on both sides, so far at least as it is presentable in words, and intelligible to one who is not a biologist, he rejects Darwin; he does not accept Mivart, and thinking the mystery of the origin of species unexplained, he can give no hint towards its solution. We should counsel the young naturalist neither to reject nor yet to accept Darwinism, but to use it tentatively and provisionally.

One feature in Mr. Maclaren's work we have great pleasure in noticing. He writes as a scholar and a gentleman. Nowhere does he impute dishonourable motives, and nowhere does he appeal to the *odium theologicum.* Where he finds his authorities using this assassin's weapon he leaves them.

Memoir and Correspondence of Caroline Herschel. By Mrs. JOHN HERSCHEL, London: John Murray.

BIOGRAPHIES, as a class, are not the most satisfactory of literary performances. Sometimes the author is led by natural affection or especial interest to overrate the importance of his hero. Sometimes, as in books of travel, the subject is used as a mere occasion for self-display on the part of the writer. From both these faults the work before us may be pronounced free. It brings before us a personage who is well worth knowing, and its authoress suppresses her own individuality in her theme.

Every student of that important question, the "heredity of genius," must be aware that the Herschel family affords a valuable affirmative instance. Two of its members, Sir William and Sir John, have taken a position in science on which it is quite needless to enlarge, and the present inheritor of the name may be said to be following in the footsteps of his illustrious father and grandfather. But it is not so generally known that the father of Sir William Isaac was a meritorious musician, and his grandfather, Benjamin, an eminent landscape gardener; nor that the founder of the family, in company with his two brothers, left Moravia in the early part of the seventeenth century for the sake of his religion, and settled in the north of Germany. This latter fact, though it gives no evidence of extraordinary intellect, is yet a convincing proof of that fixity of purpose without which the most splendid intellect rarely leads to any tangible results. Nor is it widely known, even in cultivated circles, to what an extent Caroline Lucretia Herschel, the sister of Sir William, shared his genius, participated in his studies, and contributed to his glory. This general ignorance the work before us will remove. It has the additional merit of throwing much novel light upon the career and character of the great astronomer himself, of whom, as the authoress informs us in the preface, no good biography is in existence.

The career of Caroline Herschel, though prolonged to the unusual term of ninety-eight years, and bridging over the interval from the Seven Years' War to the Third French Revolution, offers little of a striking nature. It is throughout an example of "plain living and high thinking." She was born at Hanover in 1750, followed her brother to England in 1772, and took up her residence at Bath, where he was then established as a music-master. His professional avocations, however, were but the means to an end. Every spare moment was devoted to astronomical research and to the improvement of the necessary instruments. In this arduous undertaking his sister rendered him the most essential service. How severe were his early struggles, and how great the difficulties with which he had to contend, may be learnt from this memoir. "My only reason," says Miss Herschel, in a note addressed to her nephew, the late Sir J. F. Herschel, "for saying so much of myself is to show with what

miserable assistance your father made shift to obtain the means of exploring the heavens." It would be unfair to the author to quote the details of the steps by which Sir William Herschel attained recognition, and of the share of merit due to his noble and devoted sister. As we find truly remarked in the Preface, "great men and great causes have always some helper of whom the world knows little. Sometimes these helpers have been men, sometimes (more frequently, in our opinion) they have been women who have given themselves to help and to strengthen those called upon to be leaders and workers, inspiring them with courage, keeping faith in their own idea alive, in days of darkness,

'When all the world seems adverse to desert.'

"Of this noble company of unknown helpers Caroline Herschel was one."

Surely it is, then, but right that she should receive her due share of honour. In this volume the reader will learn how "it was owing to her thrift and care that he was not harassed by the rankling vexations of money matters, how she had been his helper and assistant when he was a leading musician; how she became his helper and assistant when he gave himself up to astronomy. By sheer force of will and devoted affection she learned enough of mathematics and of methods of calculation, which to those unlearned seem mysteries, to be able to commit to writing the results of his researches. She became his assistant in the workshop, she helped him to grind and polish his mirrors, she stood beside his telescope in the nights of mid-winter, to write down his observations when the very ink was frozen in the bottle. She kept him alive by her care; thinking nothing of herself, she lived for him." So she stood by his side for fifty years, and when the musician of Bath had become Royal Astronomer, famed as the discoverer of Uranus and maker of the most powerful telescope then known, and had passed away full of years and glory, she returned to her native country to die. Even to Germany we almost grudge the honour of giving her a grave. Profoundly, however, as she felt her brother's loss, and deprived as she was of what had been the great purpose of her life, she survived him for nearly twenty-four years, still interested in astronomical research, and almost inclined to accompany her nephew on his well known expedition to South Africa. Her letters during this period are extremely interesting. On receipt of the Astronomical Society's medal she even says: "I felt more shocked than gratified by that singular distinction, for I know too well how dangerous it is for women to draw too much notice on themselves." In a letter to her nephew she uses these memorable words: "Whoever says too much of me says too little of your father."

Nevertheless we hold that very much may be justly said in her praise without in the least detracting from the reputation

of her brother. She regretted frequently that, in the first bitterness of her bereavement, she had left England. But it is very clear that had she remained in this country she would have found no satisfaction in the present. She looked upon progress in science as so much detraction from her brother's fame, and even her nephew's researches might have become a source of estrangement had she remained with him. She died early in 1848, and, singularly enough, the funeral solemnities took place at the same garrison church in which she had been baptised, nearly a century before. Royal Hanoverian carriages took part in the procession, and the coffin was covered with palm branches sent from Herrenhausen by the Crown Princess.

One most valuable lesson may be gathered from this book. Caroline Herschel did not think it needful to attitudinise on a public platform and cry out to all the winds of heaven that she "might, could, or would" do great things if not restricted by "male jealousy from graduating honours." An idle plea this, everywhere—idlest of all in England, where so many of our mightiest minds have no connection with our national universities, whether as students or professors, and only take degrees when they confer instead of receiving honour by the acceptance.* She went to work, and found no obstacle, either from laws or from social prejudice. Men saw that she was genuine, and honoured her accordingly, just as, on the other hand, men worthy of the name laugh at the "shrieking sisterhood," knowing it to be a sham. So far from self entering into the mind of Miss Herschel, she even declared, "I am nothing, I have done nothing; all I am, all I know, I owe to my brother. I am only the tool which he shaped to his use—a well-trained puppy-dog would have done as much." Such unconsciousness of its own claims and merits is often the companion of true genius. Great discoverers have said that any man, with patience and perseverence, might do all they have done. Not the less is it a mistaken estimate. Only a mind similar in its powers to that of her brother could have been trained to aid in researches like his. A lady who had discovered eight comets and effected the reduction of the places of 2500 nebulæ, who was the worthy recipient of the gold medal of the Astronomical Society and of the Gold Medal for Science given by the King of Prussia, accompanied by a letter from Alexander Humboldt outweighing a score of degrees and diplomas, might well have claimed an independent position in the scientific world. That she was content to merge her own glory in her brother's, and to live only for him, is a touching

* Many foreign critics misjudge English science on this very account. As in their own country the universities are the focus where all the greatest thinkers, the "bedeutendeste Maenner" are collected together, and whence scientific discoveries radiate out to the world, they expect the same in England; and, finding nothing in Oxford or Cambridge like what emanates from Goettingen, or Heidelberg, or Bonn, they are led to think that we are, as a nation, producing nothing.

instance of the grand simplicity of her character. It is well that he was, at once intellectually and morally, not unworthy of such a self-sacrifice.

This memoir will, we are convinced, be found pleasant and profitable reading, not by astronomers merely, or even by students of science, but by a much wider public. It will be wholesome, in these days of noisy self-glorification, to be brought into communion with a mind so pure and elevated as that of Caroline Herschel.

The Natural Foundation of Religion. By JAMES SAMUELSON. London: Longmans, Green, and Co.

WE have here a small, but pithy work, on an important and difficult subject. The author, seeking to "adapt his language to the capacity of the large class of intelligent thinkers whose daily avocations leave them little time for metaphysical studies," brings forward evidence of the existence of a personal and intelligent, and we presume consciously intelligent, Deity. His method is what is generally known as the Paleyan, or design, argument. He considers, in succession, modern doctrines and natural theology, ancient faiths and universal belief, matter and force; how the universe differs from other mechanical contrivances; the progression of nature; universal order produced by an intelligent will; the belief in the existence of an ordering intelligence practically universal; the mysterious nature of the universal intelligence the cause of extreme unbelief and of unreasoning faith; the possibility of forming a clear but limited conception of the universal intelligence; evidences of the existence of the Deity in nature; protective resemblances; mechanical appliances of insects; correlations of insects and plants; artificial and natural selection; the presence of useful minerals; the negative influence of scientific discovery on religion; obstacles to the belief in the Deity from natural evidences; the "matter and force" controversies; the intelligent employment of natural forces by man; the conception of the Deity necessarily limited, but expansive; summary of subjects of physical research affording evidence of existence and action of the Deity; the study of mankind; extended scientific knowledge beneficial to the cause of religion; changes in progress; interchange of thought between clergy and laity; modern incentives to the pursuit of wisdom,

These topics are handled clearly and fully, as far as the limited space will allow. There is a praiseworthy absence of what is known as "padding," and an amount of candour still more praiseworthy. The author is not one of those well-meaning but narrow souls who pronounce every new view concerning the

modus operandi of the First Cause necessarily atheistic. On the contrary, he finds in the nebular hypothesis, and in the doctrine of organic evolution,—even in its Darwinian phase,—fresh evidence for the existence of an intelligent superintending power.

At the same time we doubt in how far his arguments will prove satisfactory, either to atheists or to that more numerous class who—whilst firmly believing in the existence of Deity—have found the inquiry into final causes unsatisfactory, if not delusive. To take an instance, the author, speaking of useful minerals, makes the following observations :—" We have found in operation, before man's advent, ' improving ' processes similar to those which he employs for the attainment of his aims and purposes. And not only does man carry on artificial processes resembling the natural operations of the past and present, but we know that there are deposited low down in the earth's strata numerous materials whose existence has been disclosed by his intelligence, and of which the sole apparent purpose is to afford him the means of self-improvement. We may theorise, with more or less plausibility, concerning the mode in which coal was formed and deposited in the carboniferous strata, but there are two facts in connection with its presence there which cannot be disputed. One is, that coal is not a decorative object, as are trees and flowers, and that (so far as we can judge) it in no way contributes to the stability of the earth's crust, and the other that its use is to help us in a thousand different ways. The same reasoning applies to iron, and other valuable and useful metals, as well as to many well-known minerals, such as sulphur, salt, and petroleum, for the presence of which we can find no other justification than their usefulness to man. To say that they may have some other purpose unknown to us would be of no avail, for if it be worthy of consideration it must be a more intelligent purpose than that of serving man ; and if those substances have been placed where they are, as who can doubt, for our uses, then they afford unmistakable evidence of the sympathy existing between the Intelligence which caused them—no matter how—to be deposited under the surface long ages since, and mankind of to-day. But even this phenomenon, although it was brought about by the apparently blind forces of Nature long before man existed to understand and appreciate its value to himself, is no less the act of an intelligent Providence than is the paternal forethought of a father who ' lays by ' a provision for the education of his children before they come into the world. As in the other cases cited, it is merely a question of degree ; and that a human parent does not always adopt the wise precaution referred to arises from the inferiority of his intelligence as compared with the ' Infinite Intelligence,' or to some other imperfection inherent in human nature."

Now, no one certainly will deny that coal helps us " in a

thousand various ways." But in every one of these ways we are hindered, injured, or annoyed by one of its ingredients. All true coal contains sulphur, sometimes as little as a pound in the ton and sometimes exceeding a pound in the hundredweight. If we burn it in our domestic fires or in the furnaces of our steam-engines we fill the air with fumes of sulphurous acid, which is taken up by atmospheric moisture, and descends upon the ground in the form of a corrosive rain, blighting and destroying vegetation. If we use it in metallurgical operations this same sulphur injures the quality of the metal obtained. We employ the coal in our gas-works, and are still haunted by the same enemy. The gas must be purified at considerable expense, and even when all has been done that science and experience can suggest, it still retains traces of sulphur. Hence if burned in our dwellings, shops, or warehouses, it injures the colours of textile goods, weakens their fibre, and damages books, pictures, steel-wares, and a long catalogue of other substances. It may be said that we may some day remove the sulphur from the coal. But we must remember that it is too small in quantity to render such an attempt remunerative, or even self-supporting; that it is present in a form not soluble in any cheap liquid, and that it is disseminated in minute particles through the entire mass of the coal. If we apply heat we drive it off, and decompose the coal at the same time. In short, to remove it without injury to the fuel, and without seriously enhancing its price, is a problem whose solution is not even conceivable. So long as we regard coal as a something existing without any special reference to man, and of which he avails himself, taking, in the common phrase, "the rough with the smooth," all this may cause regret, but not surprise. If, on the other hand, we are told to view it as a something especially prepared for man's use, by a Being of infinite power and wisdom, we are at once staggered. Were coal the product of a finite, imperfect, human intelligence, we should pronounce it useful, certainly, but very faulty. And finding it thus faulty, can we, without irreverence, proclaim it in the sense the author takes, the work of Infinite Wisdom?

This instance of coal is no isolated case; our sulphur ores are contaminated with arsenic, our iron ores with sulphur and phosphorus, all generally in quantities too small to be of value, or to cover the process of separation, but quite sufficient to deteriorate the bodies which they thus accompany. Such facts will always incline many minds to turn away with sadness from the study of final causes, feeling that they can afford no satisfactory evidence of the rule of a Being at once all-powerful, all-wise, and all-beneficent.

Geology for Students and General Readers. Part I., Physical Geology. By A. H. Green, F.G.S., Professor of Geology in the Yorkshire College of Science, Leeds. London: Daldy, Isbister, and Co.

We have here the first part of a treatise on geology, designed for the student and the general reader, and embracing that portion of the science distinguished as physical geology. The author declares his purpose to have been the compilation of "a manual which would serve the purpose of those students who, without going very deeply into the subject, desire to know as much of the science as any man of culture may be reasonably expected to possess." We devoutly wish that every man of culture, so-called, had half the acquaintance with geology which might be gathered from this work. But unfortunately gross ignorance of geology, and, indeed, of every branch of physical science, is tolerated not merely in private men of culture, but even in literary characters,—the teachers and guides of the public. Arago well observes: "Under the brilliant and superficial varnish with which the purely literary studies of our colleges almost necessarily invest all classes of society, we generally find—let us be brief—a complete ignorance of those beautiful phenomena, of those grand laws of nature which are our best guard against prejudice." We scarcely think that Mr. Green takes a sufficiently high ground in maintaining the value of natural science "as an instrument for training the mind to reflect and reason." Natural science alone can teach us to deal with things, and to draw right conclusions from facts rightly observed. The mere mathematician—strange as the assertion may sound—is a wretched reasoner, because he invariably begs the question in a series of baseless initial assumptions. His arguments bristle with "if we only suppose," "let us assume," "let it be granted," &c. Upon these assumptions and suppositions, sometimes false and still more often doubtful, he builds a superstructure which, however correct in itself, is of necessity worthless. That the study of the classics should ever be considered superior to that of natural science as "an engine for developing the reasoning powers," is an old superstition whose origin it is not difficult to perceive, and which must ultimately fade away.

As a matter of course, in a work like this, intended for the use of the general public, originality of speculation is not to be demanded. The author declares that he has "borrowed right and left," and that he doubts "whether there is in the book, from beginning to end, that can be said to be new." But though avowedly a compilation, it is one which only a master of the subject could have produced. If it contains little that is new, it contains less that is not true, and the student may find here a solid foundation. The best authorities have been followed as

regards matters of fact, and in the more speculative part of the subject the author shows great discretion. Commencing with an account of the aim and scope of geology, he passes on to a brief history of its rise and progress. He next enters upon descriptive geology, discussing in succession—denudation, the destiny of the waste produced, the method of formation of stratified rocks, the definition and classification of derivative rocks, and the method of determining the physical geography of the earth at different periods of its past history. The volcanic, metamorphic, and granitic rocks are next described, followed by a consideration of the questions how the rocks came into the positions in which we now find them, and how the present surface of the ground has been produced. In all these chapters, whilst cultivating brevity, Prof. Green not merely gives the reader results, but explains very clearly the logical processes by which such results have been reached. In the last two chapters he deals with the present physical condition of the earth, the causes of upheaval and contortion, the origin of the heat required for volcanic energy and metamorphism, examining the hypothesis of a thin crust, and reviewing the theories of Mr. Hopkins, Mr. Scrope, the Rev. O. Fisher, Prof. Sterry Hunt, and Mr. R. Mallet.

Mr. Green's concluding remarks on speculative geology may be pronounced eminently judicious. He declares that we are unable to pronounce positively upon the present state of the earth's interior, and that though the arguments in favour of a thick crust are very weighty, they are by no means conclusive. As to the vexed question of geological time, he points out a very important objection to the calculations of Sir W. Thomson, leading, as is well known, to a result far shorter than what geological and organological phenomena evidently require. Sir W. Thomson, starting from the basis of the Nebular hypothesis, assumes that the earth has been, and will be, cooling all along. Mr. Lockyer, however,* agreeing with the views of Prout and Dumas—and we may say, to a certain extent, of Mr. Ennis, as laid down in his work on the "Origin of the Stars," which will be noticed in our next issue—points out a very obvious method "by which the failing heat may have been replenished perhaps over and over again." Many of the substances provisionally regarded as elements, merely because our resources have been so far unable to effect their decomposition, may in reality be compounds formed from constituents which, during the early part of a star's career, existed in a free state. But when the temperature of the heavenly body was so far reduced as no longer to keep these primeval elements in a free state, then combination took place, liberating a very considerable amount of heat. "Thus," Mr. Green continues, "the life of a star may not have been one con-

* See CHEMICAL NEWS, Oct. 3, 1873.

tinuous process of cooling, but it may have every now and then fired up afresh, and the time taken to reduce it to a certain temperature may have been much longer than if it had gone on always steadily losing heat." We may add that this view agrees not badly with certain well-known astronomical phenomena. Nor should the very untrustworthy nature of mathematical speculation, where not constantly controlled by an appeal to actual facts, be left out of view. If mathematicians were till lately in error to the extent of 1-30th part in such a comparatively simple matter as the distance of the earth from the sun, what weight can we seriously attach to Sir W. Thomson's speculation as to the past direction of the solar system?

In the dispute between the Uniformitarians and the Catastrophists, Mr. Green takes an intermediate position. He holds it highly probable that "when the earth was hotter than it is now all the phenomena which depend directly or indirectly on the internal heat, such as metamorphism, volcanic energy, and contortion, must have been proportionally more energetic; and if the sun was at the same time hotter, all the geological operations depending on meteorological conditions, such as denudation, must have gone on faster and on a larger scale than now." Still he holds that, for the period over which our researches extend, Uniformitarianism may be practically correct.

As regards the climatic changes which the earth has undergone, such as the glacial epochs and the genial Miocene period, our author inclines to the views expounded by Mr. J. Croll, in his "Climate and Time in their Geological Relations: a Theory of the Secular Changes of the Earth's Climate."

We shall look forward with interest to the appearance of the second part of the work before us, and we think we may safely congratulate the Yorkshire College of Science on including in its professorial staff so able an expounder of a fascinating and important science.

An Introduction to Animal Morphology and Systematic Zoology. By ALEXANDER MACALISTER, M.B., Professor of Comparative Anatomy and Zoology, University of Dublin. Part I.: Invertebrata. London: Longmans, Green, and Co.

THE author of this work states in his Preface:—"In teaching zoology and comparative anatomy I have found that students desire to have a text-book in their hands to enable them to learn the terminology of the science, and by giving them a connected view of the varieties of animal forms to assist them in remembering the practical instructions of the class-room."

This want he has endeavoured to meet, and, in our opinion, with good success. Of course minute details must not be looked

for in a work which aims at giving a general view of morphology, a sketch of animal geography, and a structural survey of the Invertebrates from the Protozoa up to the Hymenoptera, within the compass of some 400 pages. But the space at command has been skilfully utilised. Perhaps the least commendable feature of the book is the intensely technical character of its language, which in many passages must be simply unintelligible to the private student. It has often struck us that the Germans have a great advantage in the circumstance of having at command technical terms formed from their own language and not borrowed from the Latin and Greek. The lesser flexibility of the English tongue prevents our imitating them fully in this respect, but we are confident that in many cases plain English terms might be found to supersede the foreign terminology now in vogue, and which is upheld by a prejudice resembling that which induces physicians to write their prescriptions in Latin, and lawyers to cultivate a peculiarly incomprehensible jargon.

We are glad to find that Prof. Macalister avoids the Cuvierian error of placing the mollusca above the articulata. We think that there is evidence sufficient to warrant us in regarding the antennæ of insects as organs of smell, even if that should not be their only function. Like all authorities on animal classification, Prof. Macalister fails to see that the so-called orders of insects have a strong claim to be regarded as groups of a higher rank. The conflicting views on the origin of species are briefly stated, without any account of the arguments on either side. The important truth that species, genus, &c., must be looked on as more or less arbitrary, ideal conceptions, is clearly brought forward. Into descriptive natural history the author, of course, cannot enter; but one fact, incidentally mentioned, may interest our readers. The arm of a cephalopod of the genus Architeuthis —which would be commonly called an octopus, or sea-devil— driven ashore on the west coast of Ireland in the year 1875 measured 30 feet in length. This, as the author hints, furnishes some basis for the old Scandinavian legend of the Kraken.

For all students who are in a position to have its technical language explained this work will prove a most valuable textbook. For the benefit of those less favourably situated we should venture to suggest the addition of a glossary.

Bubbles from the Deep. By ARTHUR GREAVES. Halifax (Nova Scotia): M. A. Buckley.

ON receiving this little volume we were somewhat perplexed at its title. However, in these days the name of a book is not always intended to throw any light upon its subject. So we

commenced an examination of the work, expecting a report on the results obtained by deep-sea dredging. But to our surprise, we found that the contents were poetry. We were, therefore, about to lay the book aside, leaving it for more experienced and competent judges to pronounce on its merits, when we accidentally came upon a kind of appendix. In this figured a correspondence between the author and a real or imaginary friend rejoicing in the uncouth name of "Phineas Phillgro." This latter worthy asks for the privilege of annexing to the book an effusion of his own, and unfortunately receives Mr. Greaves's consent. The addition thus made is a piece of wretched doggrel, expounding the loathsome doctrine known by the scarcely less loathsome name of "miscegenation." Mr. Phillgro thinks that the Aryan race in America is decaying, and proposes its resuscitation with the "rich and luscious blood of the tropics." Why not go a step further? Is it not just possible that the blood of the gorilla or the chimpanzee might prove richer and more luscious still? But, in all sober sadness, if any part of the Aryan race, whether in America or Europe, is degenerating, is it not better to attempt its restoration, not by unions which it is revolting to contemplate, but by the removal of the causes which have led to such degeneracy? These causes are many, and well known. An intermixture of different branches of the Aryan race may have had beneficial results. A fusion of different "races"—if this be the legitimate term—has proved disastrous wherever tried. What strain, for instance, is viler than the half-breeds of Macao? We should seriously advise Mr. Greaves, if he wishes his poems to circulate in the land of his forefathers, to renounce "Phineas Phillgro and all his works.

PROGRESS IN SCIENCE.

PHYSICS.

An Address on the "Fundamental Principles of Scientific Arctic Investigation," was delivered, in September last, before the 48th Meeting of German Naturalists and Physicians at Graz, by Lieut. Charles Weyprecht. The author argues that the scientific results obtained by former expeditions bear no comparison to the enormous sums expended upon them. Within the last fifty years England and America alone have sent out more than twenty-five large or small expeditions, at a cost of far beyond £1,000,000 sterling. The most important scientific results of this long series of costly expeditions are —the discovery of the magnetic pole, the determination of the physical constants for a number of points, a more extended knowledge of the Natural History of high northern regions, and, finally, the topography in detail of a cluster of islands of little importance. But the gains to Natural History are locally far too limited considering the number of voyages, nor have the researches been systematically conducted, while the physical observations—owing to the manner in which they have been made—offer us little more than unconnected average values, which, through local influences and annual fluctuations, neither to be avoided nor overcome, possess less value than generally has been accorded to them. We are still wanting from the Arctic regions even one series of observations upon the disturbances of the three magnetic elements. The data furnished by the Expeditions are confined to absolute determinations—sadly wanting in accuracy, because exposed to every casual disturbance—and to observations upon variations of declination. The disturbances of the horizontal intensity and of inclination have been utterly neglected. Of the relation in which the horizontal and vertical components of the earth's magnetism stand to each other during the disturbances we know nothing, and are therefore unable to decide whether the total force may not rather simply change in direction, and not in degree of strength. As to intensity and inclination, there is not a point from which we possess sufficiently accurate data to serve, after a lapse of years, as a basis for determination of secular changes. We might almost say that we know not much more of Nature's doings, in high northern and southern latitudes, than just enough to show us how important a thorough scientific investigation of these regions is to natural philosophy in all its branches. If we enquire why the scientific results obtained are so scanty, we discover that the fault lies less in the observations made than in the generally false principles on which hitherto Arctic Expeditions have been sent out—principles which, in most cases, have actually been direct hindrances in the way of true scientific research. The grand fault has been that the first object of almost all the Expeditions has been geographical discovery. The investigation of those vast unknown regions about the Poles will and must be pursued, regardless of cost of money and human life, so long as man makes any pretension to progress. But its great object must be a nobler one than mapping and naming icebound islands, bays, and promontories, in this or that language, or reaching a higher latitude than any predecessor. Descriptive geography neither can nor should be excluded, but must not stand as the object first in rank: in uninhabited and uninhabitable latitudes, which by force of their physical conditions are important for science alone, it has value only in so far as the meteorological, physical, and hydrographical phenomena of the earth are influenced by the character of the land; broad and general sketches therefore suffice. The geographical object of former Expeditions must bear the blame that the stations of observation are crowded so one-sidedly within a single region. In the search for a North-West passage, and

in the attempts to reach the North Pole, the same routes have been continually adopted with little variation, and all else overlooked. To be sure some of those Expeditions had specially the sad object of seeking the remains of Franklin's disastrous Expedition. In these, the employment of the sledge attained that extraordinary development which has excited such universal wonder and imitation. But where the sledge stands out prominent, scientific research can play but a secondary part. The journeys occupy the best time in spring and autumn, and never admit of that repose which is essential to thorough observation. To what extent scientific research has been neglected in the greed for discovery, the best proof is that it is only two years that the first party has wintered for purposes of science in the Archipelago of Spitzbergen, geographically well known and approachable even in 80° lat. every year almost without obstacle, although these islands form one of the most important and favourably situated points for observation in the Arctic regions. It is to the Expeditions to Spitzbergen and Siberia, fitted out at comparatively small cost, that we owe our most thorough studies of the flora and fauna of this and the antediluvial world, of the effects of the Arctic conditions on animal and vegetable life, &c. A second cause of inadequate results lies in this—that all Expeditions have been single and independent, and afford no synchronous observations for comparison. Where and whenever the forces of Nature, and the physical phenomena which they produce, are the object of study, simultaneous observations on many points are a fundamental condition of success. In peopled countries, this condition in a degree fulfils itself over the greatest possible extent of surface, through the multitude of chance observers. In the Polar regions the observer must depend on himself; the simplest and most important data are wanting; for instance, the extent of territory covered by a phenomenon. In a far higher degree is this true in regard to those phenomena not perceptible by the senses, and only perceived by the aid of instruments. Nothing but simultaneous and most careful observations, made at numerous stations more or less widely separated, can yield decisive results. After Gauss and Weber had introduced the synchronous magnetic *termdays*, the science of Terrestrial Magnetism very soon burst the narrow bounds, in which until then it had been restrained. Animated by their success, England established her colonial observatories, and by them proved the subjection to natural laws of all magnetic phenomena; but none of these stations reached the Arctic regions, the most northern being in latitude 61°. However interesting and important their observations are, still they do not suffice to give us that view of the joint action of the combined forces of terrestrial magnetism in high latitudes—the extensive home of disturbance—which is absolutely indispensable to a sound theory. They leave us in the dark as to the position of the centres of disturbance, as to the limits of particular movements, as to their synchronism at different distances as to the manner in which the separate oscillations exhibit themselves along the same parallel in different longitudes. Hence all conclusions as to the influence of local circumstances upon the strength and character of the disturbances fail. The English observations have proved that perturbations at various places in various years cannot be compared, since, for instance in Toronto, they, in one year, amounted for the declination to three times, and for the horizontal intensity to six times, as much as in another. It would therefore lead to utterly false conclusions if the Toronto, Sitka, or Athabasca observations of different years be compared in the matter of intensity. What may be said of magnetic disturbances is equally applicable to the northern light. There are many reasons to believe that this phenomenon in high northern latitudes has but a very local character, a point only to be decided by synchronous observations. For here also it would be false to compare different years at different points with each other. Different places the same year, or different years for the same places, can only be compared. Through the frequent neglect of this axiom in analysing auroral phenomena many an error has crept in. The entire meteorology of our day rests upon comparison; all the successes of which it can boast—the laws of storms, the theories of the winds—are results of synchronous observations. The average values of the meteorological con-

stants of particular places are most important to the knowledge of the physical conditions of the earth, but they suffice no longer as soon as the question is—What are the laws which govern the changes to which they are subject? They can answer the *how*, but are rarely equal to the *why*. The accessibility of the Arctic interior from different quarters is constantly discussed in all scientific circles. It has been common to draw conclusions from the favourable or unfavourable experiences of Arctic voyagers by different routes and at different times, which conclusions have afterwards proved to be mistaken, because the difference in the condition of the ice in different years has not been taken into account. In the years 1871 and 1874 the ice on the same meridian near Nowaja-Zemlja began at 78° lat., but in 1872 it reached 6° farther south. Now, it is probable that on the opposite side of the Arctic basin—the American coast—the case during this period was precisely the reverse. But this we cannot decide with certainty, because we do not know whether the exceptional increase of ice on this one side is necessarily conjoined with a decrease on the other; or whether perhaps the Arctic basin does not contain totally different amounts of ice in different years. We need that general view of the entire mass of ice and its movements which can only be gained by simultaneous observations at various points. One-sided judgments, formed at a single point, whose conditions depend on the accidents of the year, will never enable us to draw correct conclusions as to the accessibility of the Arctic interior. For the descriptive branches of science synchronism is of less importance: these demand continuous systematic study. "Voyages have enlarged the catalogues," Prof. C. Vogt writes me, "but only continued observations on the spot produce the deeper scientific results." To this the unscientific man is inadequate, however industrious a collector he may be. If our object be genuine progress in Natural History, the aid of the man of science is absolutely necessary, which we have had in but few instances. In view of the ever-increasing interest in Arctic research, and of the readiness with which governments and private individuals are continually furnishing the means for new Expeditions, it is desirable to establish the principles on which they should be sent out, so that their utility to science may be in proportion to the great sacrifices made, and they be relieved of that adventurous character which can only be prejudicial to science. The following points, the author considers, will meet the requirements set forth above:—

1. Arctic research is of the highest importance to the knowledge of Nature's laws.
2. Geographical discovery in those regions has a higher value in so far only as it opens the field to scientific research in the narrower sense of the term.
3. Arctic topography in detail is but of secondary importance.
4. The geographic pole has for science no greater significance than any other point in the higher latitudes.
5. Stations of observation are—without regard to their latitude—the more favourable in proportion to the comparative intensity of the phenomena under investigation.
6. Independent series of observations have but secondary value.

It is not necessary to extend our sphere of observations into the very highest latitudes in order to secure scientific results of the greatest importance. For instance, stations at Nowaja-Zemlja (76°). Spitzbergen (80°), East or West Greenland (76°—78°), North-America east of Behring's Strait (70°), Siberia at the mouth of the Lena (70°), would give us a zone of observation quite around the Arctic regions. Greatly to be desired are stations near the centres of magnetic intensity. The observations there would be connected with our own through the stations already established near the Polar circle, which only need to be strengthened. The means expended on any one of the more recent attempts to reach the highest latitude would be amply sufficient to sustain all these stations for a year. The object of these Expeditions would be, with instruments precisely alike, governed by precisely the same instructions, and for a period of one year at least, to record a series of the utmost possible synchro-

nous observations. Attention should be directed above all to the various branches of physics and meteorology, as being of the highest degree of importance; then to botany, zoology, and geology; and, lastly, to geographical detail. Should it be possible to establish, in connection with these Arctic stations of synchronous observations, one or more in the Antarctic regions, we might expect results of inestimable value.

Several important papers have been read before the Physical Society since the publication of the April number of this Journal. At the meeting on April 8th Prof. G. Carey Foster, F.R.S., described an instrument which he has constructed for illustrating the law of refraction. It is founded on the well-known method of determining the direction of the ray after refraction by means of two circles described from the point of incidence or centre, the ratio of whose radii is the index of refraction. A rod representing the incident ray is pivoted at the point of incidence, and projects to a point about 4 inches beyond. To this extremity is attached a vertical rod, which slides through a nut in another rod also pivoted at the point of incidence. The lower extremity of the vertical rod is attached to a link, so fixed as to constrain it to remain vertical. By this means the two rods always represent respectively the incident and refracted rays, and the index of refraction can be varied by altering the position of the nut through which the vertical rod passes on the rod to which it is attached.

At the same meeting Prof. Foster exhibited a simple arrangement for showing the interference of waves, and a method—suggested by Prof. Kundt—for showing, in a simple manner, that the air in an organ-pipe is in a constant state of alternate condensation and rarefaction.

At the meeting on April 29th the Secretary read a communication from Sir John Conroy, Bart., "On a Simple Form of Heliostat." The author substitutes two silvered mirrors for the looking-glasses usually employed, and he has shown that the loss of light with this arrangement is less than when the light is once reflected from a looking-glass.

Mr. S. P. Thompson described some experiments which he had made on the so-called "Etheric Force." If the secondary current from an induction coil be used instead of a current direct from the battery the effects are much more marked. The secondary current of a Ruhmkorff's coil is made to traverse a short coil of wire, which is thoroughly insulated from the internal core, and into the circuit an arrangement is introduced by means of which the spark may be made to traverse a variable thickness of air in its course round the short coil. It is found that if this spark is very short the spark obtained from the internal core is also short; but as we increase the thickness of air to be traversed, the spark which may be drawn off increases. The greatest effect, however, is produced when one terminal of the coil is connected with the earth, the spark then obtained being about ½ an inch in diameter. Mr. Edison considered that the spark was retro-active, but Mr. Thompson showed by an experiment that deficient insulation might lead to such a conclusion. He then proceeded to show that just as the charge given to a gold-leaf electroscope is at times positive, and at times negative, without any apparent reason for the change, so, if the core of the arrangement employed be connected with a Thomson's galvanometer, the needle will be found to wander irregularly about the scale on both sides of the zero. In order to show that these experiments are identical with those conducted as originally described by the discoverer, the terminals of the induction coil were connected with the coil of an electro-magnet, the same means of including a layer of air in the circuit being introduced. The effect in this case was found to be precisely similar to that obtained with the special arrangement previously used. With a brush discharge a Geissler's tube could be illuminated, and when the layer of air was infinitesimal the spark produced was also infinitesimal. It was then shown that if the spark at the point of contact in the key, when a direct battery current traverses the coil, be done away with by shunting the extra current which gives rise to it, no spark can be obtained from the core. It thus appears that no spark is obtained when there is no necessity for an inducing current to

accumulate until it has sufficient tension to leap over a resisting medium, and that, as the thickness of this resisting medium increases, the spark obtained becomes greater. Evidently, on these occasions, the charge has time to attract unlike and repel like electricity in the core, and if a conductor in connection with the earth be presented to this core the like electricity will escape: hence a spark will result. As soon, however, as the tension has become sufficient to leap over the layer of air, it will be necessary to restore equilibrium in the core. Hence there will be a return spark in the opposite direction. From these experiments Mr. Thompson concludes that the phenomena observed may be explained by the ordinary laws of induction.

On June 10th Mr. W. J. Wilson explained a reflecting tangent galvanometer, which he has recently designed, for the purpose of exhibiting the indications of the instrument to an audience, and so arranged that the divisions on the scale show, without calculation, the relative strengths of different currents. The beam of light, after passing through a small orifice traversed by cross wires, is reflected vertically by a fixed mirror; the ray then passes through a lens, and is again reflected from a small plane mirror parallel to the first, which is rigidly fixed below a small magnetic needle. By this means the ray becomes again horizontal, and, since the light now falls on the second mirror always at the same angle, the extent of motion of the ray is identical with that of the needle, and, if the scale be one of equal parts placed in the magnetic meridian, the indications on it will be proportional to the tangents of the angles, and, therefore, to the strengths of the currents. The needle and mirror are suspended by a silk fibre, and a bent strip of aluminium, the ends of which dip into water in an annular trough, is attached to the needle in order to check its oscillations. A series of observations, taken with varying resistances introduced into the current, showed that the indications are very reliable.

Prof. G. Fuller, C.E., described his "Electric Multiplier," an instrument which may be looked upon as an automatic electrophorus. An insulated plate of vulcanite is supported in a vertical position, and on each side of it is an insulated metallic plate, and these can be moved together to and from the vulcanite by rotating a handle. When these plates are far apart, two metallic arms, provided with points, are made to pass one on each side of the vulcanite plate. One of these is insulated, and is provided with a rod terminating in a knob, which at a certain point in its path almost touches the metallic plate on the opposite side of the sheet of vulcanite. The other arm is in connection with the earth. The action of the instrument is as follows:—A charge of, say, negative electricity having been given to the insulated arm, it is passed over the face of the vulcanite, while positive is drawn up from the earth and thrown upon the opposite face by the uninsulated series of points. These arms are then removed, and the two metallic plates are brought into contact with the vulcanite. Call the side of the plate charged with negative electricity A, and the other B. The negative of A induces positive on the near face of its metallic plate, and repels the negative. This passes, by a strip of tin-foil joining the two faces of the vulcanite, to the other metallic plate, neutralising its free positive; and when the plates are moved away from the vulcanite, that from A is charged with positive, and that from B with negative. Before reaching its extreme position, this latter communicates its charge to the insulated arm by the brass knob, and the electricity is then distributed over the face A. At the end of its path, B is momentarily connected to earth. It will be evident that the effect of again bringing the plates in contact is to increase the charge of positive electricity on the metallic plate opposite the face A. With the small model exhibited Prof. Fuller has frequently obtained sparks an inch in length.

Prof. Guthrie exhibited Prof. Mach's apparatus for sound reflexion, which is one of an interesting series of appliances designed by him for the demonstration of certain fundamental principles in physics. It consists of a mathematically exact elliptical tray, highly polished, and provided with a close-fitting glass cover. The tray is covered with pulverised dry silicic acid, and a Leyden jar frequently discharged between two small knobs at one of the foci, when the silicic acid arranges itself in fine curves around the other focus.

• On April 28th Dr. Andrews, F.R.S., delivered a lecture before the Chemical Society "On Certain Methods of Physico-Chemical Research." The lecturer began by describing the simple form of apparatus which he employed many years ago in his researches on the heat evolved in the combination of oxygen, chlorine, bromine, &c., with other bodies. In every case the bodies to be combined were enclosed in a vessel surrounded with water, and the combination was effected either by the ignition of a fine platinum wire, or, where they acted directly upon one another, by the fracture of a glass capsule containing one of the combining bodies, the heat being measured by the rise of temperature of the water. He next referred to the arrangement by which he had been the first to decompose water so as to render visible the hydrogen and oxygen, and to measure their relative volumes, by means of atmospheric electricity and of the electrical currents from the ordinary machine. For this purpose fine platinum wires were hermetically sealed into fine thermometer tubes, which were then filled with dilute sulphuric acid by withdrawing the air by ebullition. The same current of frictional electricity will decompose the water in almost an indefinite number of such couples arranged in a consecutive series. Capillary tubes of this kind may be employed for eudiometric experiments, which would be exceedingly tedious in wide tubes. Thus oxygen gas can be at once absorbed by passing the silent discharge through it while standing over a solution of iodide of potassium. By means of the air-pump it is easy, with a gentle exhaustion, to expand the gas so that it may fill the whole tube, while the open end is immersed in the liquid which it is desired to introduce. On removing the pressure, the gas will be in contact with the new liquid. The lecturer exhibited some of the original tubes with which Prof. Tait and he first determined that ozone is a condensed form of oxygen, and explained a form of apparatus by means of which this important fact can be exhibited as a class experiment. A full description of this apparatus will be found in his lecture on ozone, which was delivered some time ago before the Royal Society of Edinburgh, and has since been published by the Scottish Meteorological Society. With this apparatus the lecturer has been able to determine that chlorine gas undergoes no change of volume from the prolonged action of the electrical discharge. His experiments on this subject have not yet been published, but they were made under singularly favourable conditions for discovering a very small change of volume in the gas, if any such change had occurred. The lecturer, in the next place, briefly alluded to the method he formerly employed for determining the latent heat of vapours, of which a detailed account was given in a former communication to the Chemical Society. The apparatus employed admits of exact experiments being made on a small scale, and consequently on substances in an absolutely pure state—an object of even greater importance in enquiries of this kind than in ordinary chemical analysis. He remarked that a large field for investigations in this part of the domain of science lay comparatively uncultivated, and would yield a rich harvest of results to any one who would enter upon it. Passing from this subject, the lecturer described a dividing and calibrating machine, which he contrived some years ago for the special work in which he has been engaged, and which has given to many of his investigations an accuracy otherwise hardly obtainable. He has been enabled by means of it to construct thermometers whose readings are absolutely coincident throughout every part of the scale, and to calibrate with almost perfect accuracy the glass tubes used in his pressure experiments. It would be impossible in an abstract to describe the construction of this machine, but it may be important to mention that the screw which moves the microscope or divider is a short one, of remarkable accuracy, constructed by Troughton and Simms. The last subject treated was the lecturer's method of investigating the properties of gaseous and liquid bodies at high pressures and under varied temperatures. By means of his apparatus, which was exhibited to the meeting, pressures of 500 atmospheres can be readily observed and measured in glass tubes—in a word, a complete mastery obtained over matter under conditions hitherto beyond the reach of direct observations. This has been effected by a novel mode of *packing* a fine steel screw, so that while entering a confined portion of water no leakage whatever

occurs under enormous pressures, and also by a peculiar method of forming a tight junction between glass and metal. The lecture was concluded by a short statement of the more important results lately communicated to the Royal Society on the properties of matter in the gaseous state.

Prof. Balfour Stewart, LL.D., F.R.S., has constructed a new instrument for measuring the direct heat of the sun. The instrument generally employed for giving the radiant energy of the sun's rays acts upon the following principle:—In the first place the instrument is sheltered from the sun, but exposed to the clear sky, say, for five minutes; let the heat so lost be termed r. Secondly, the instrument is turned to the sun for five minutes; let the heat so gained be termed R. Thirdly, the instrument being now hotter than it was in the first operation, is turned once more so as to be exposed to the clear sky for five minutes while it is shielded from the sun; let the heat so lost be termed r'. It thus appears that r denotes the heat lost by convection and radiation united when the instrument, before being heated by the sun, is exposed for five minutes to the clear sky, while r' denotes the heat lost by these same two operations by a similar exposure after the instrument has been heated by the sun; and it is assumed that the heat lost from these two causes during the time when the instrument is being heated by the sun will be a mean between r and r', and hence that the whole effect of the sun's rays will be in reality—

$$\mathrm{R}+\frac{r+r'}{2}$$

Now although this assumption may in the average of a great number of experiments represent the truth, yet in many individual cases it may be far from being true. In the new instrument the causes of variability are not allowed to operate. It consists of a large mercurial thermometer with its bulb in the middle of a cubical cast-iron chamber, this chamber being of such massive material that its temperature will remain sensibly constant for some time. The chamber with its thermometer has a motion in azimuth round a vertical axis, and also a motion in altitude round a horizontal axis. A 3-inch lens of 12 inches focal length is attached by means of a rod to the cubical chamber, so as to move with it. Thus the whole instrument may be easily moved into such a position that the lens as well as the upper side of the chamber, which is parallel to the plane of the lens, may face the sun, and an image of the sun be thrown through a hole in the side of the chamber upon the thermometer bulb. The stem of the thermometer protrudes from the chamber. A screw, somewhat larger in diameter than the bulb of the thermometer, is made use of to attach the thermometer to its enclosure, and a smaller screw, pressing home upon india-rubber washers enables the thermometer to be properly adjusted and kept tight when in adjustment. The internal diameter of the chamber is 2 inches, while the bulb of the thermometer is about $1\frac{1}{4}$ inches in diameter. The scale of the thermometer is very open, more than an inch going to one degree. Prof. Stewart has generally allowed the image of the sun given by the lens to heat the thermometer bulb for one minute, during which time an increase of temperature, not exceeding in any case 2°, has been produced. A practical objection has been provided for by Prof. Stewart. The scale being so very open, the stem comprehends only a few degrees; frequently, therefore, the temperature is such that the extremity of the mercurial column is either below or above the stem. Now the thermometer has a small upper chamber, and by means of a method of manipulation well known to those who work with thermometers, it is possible to add to or take away from the main body of mercury in the bulb so as to keep the end of the mercurial column always in the stem. But for a thermometer with such a large bulb, frequent manipulation of this kind is not unattended with danger to the bulb. To remedy this defect without altering the size of the bulb, Prof. Stewart proposes for a permanent instrument a stem, say 18 inches long, with a bore of such diameter that the stem should embrace a range of temperature between 20° F. and 92° F. Thus somewhat less than 5° will go to the inch. The stem might be protected from the risk of accident by an appropriate shield. Let such a thermometer be heated for two minutes, and the size of the lens be somewhat increased. In this case a

rise of something like 5° F. will be obtained, and this heating effect might very easily be estimated to one-hundredth of the whole, while the same thermometer would serve for all the temperatures likely to occur in these islands during the course of the year. A pasteboard cover gilded on the outside is made to surround the chamber, and between the lens and the chamber there is a pasteboard shield, with a hole in it, to permit the full rays from the lens to pass—the object of this shield being to prevent rays from the sun or sky from reaching the instrument. In such an instrument r, or the change taking place in the thermometer before exposure to the sun, will in all probability completely disappear, while r' will be extremely small. At any rate we may be quite certain that—

$$R+\frac{r+r'}{2}$$

will accurately represent the heating effect of the sun.

At a meeting of the American Academy of Arts and Sciences held on the 8th March, 1876, the Rumford Medal was awarded to Dr. J. W. Draper for his researches in radiant energy. In presenting the medals, the President (the Hon. Charles Francis Adams) referred to Dr. Draper's researches as follows:—In 1840 Dr. Draper independently discovered the peculiar phenomena commonly known as Moser's images, which are formed when a medal or coin is placed upon a polished surface of glass or metal. These images remain, as it were, latent until a vapour is allowed to condense upon the surface, when the image is developed and becomes visible. At a later period he devised the method of measuring the intensity of the chemical action of light, afterward perfected and employed by Bunsen and Roscoe in their elaborate investigations. This method consists in exposing to the source of light a mixture of equal volumes of chlorine and hydrogen gases. Combination takes place more or less rapidly, and the intensity of the chemical action of the light is measured by the diminution in volume. No other known method compares with this in accuracy, and most valuable results have been obtained by its use. In an elaborate investigation published in 1847 Dr. Draper established experimentally the following important facts:—1. All solid substances, and probably liquids, become incandescent at the same temperature. 2. The thermometric point at which substances become red-hot is about 977° F. 3. The spectrum of an incandescent solid is continuous; it contains neither bright nor dark fixed lines. 4. From common temperatures nearly up to 977° F., the rays emitted by a solid are invisible. At that temperature they are red, and, the heat of the incandescing body being made continuously to increase, other rays are added, increasing in refrangibility as the temperature rises. 5. While the addition of rays so much the more refrangible as the temperature is higher is taking place, there is an increase in the intensity of those already existing. Thirteen years afterward Kirchhoff published his celebrated memoir on the relations between the coefficients of emission and absorption of bodies for light and heat, in which he established mathematically the same facts, and announced them as new. 6. Dr. Draper claims, and we believe with justice, to have been the first to apply the daguerreotype process to taking portraits. 7. Dr. Draper applied ruled glasses and specula to produce spectra for the study of the chemical action of light. The employment of ruled metallic specula for this purpose enabled him to avoid the absorbent action of glass and other transparent media, as well as to establish the points of maximum and minimum intensity with reference to portions of the spectrum defined by their wave-lengths. He obtained also the advantage of employing a normal spectrum in place of one which is abnormally condensed at one end and expanded at the other. 8. We owe to him valuable and original researches on the nature of the rays absorbed in the growth of plants in sunlight. These researches prove that the maximum action is produced by the yellow rays, and they have been fully confirmed by more recent investigations. 9. We owe to him, further, an elaborate discussion of the chemical action of light, supported in a great measure by his own experiments, and proving conclusively—and, as we believe, for the first time—that rays of all wave-lengths are capable of pro-

ducing chemical changes, and that too little account has hitherto been taken of the nature of the substance in which the decomposition is produced. 10. Finally, Dr. Draper has recently published researches on the distribution of heat in the spectrum, which are of the highest interest, and which have largely contributed to the advancement of our knowledge of the subject of radiant energy. Through ill-health, Dr. Draper was unable to receive the medal in person. He therefore sent the following letter, which was read by Mr. Quiney:—

"To the American Academy of Arts and Sciences.—Your favourable appreciation of my researches on radiations, expressed to-day by the award of the Rumford Medal—the highest testimonial of approbation that American science has to bestow on those who have devoted themselves to the enlargement of knowledge—is to me a most acceptable return for the attention I have given to that subject through a period of more than forty years, and I deeply regret that through ill-health I am unable to receive it in person. Sir David Brewster, to whom science is under so many obligations for the discoveries he made, once said to me that the solar-spectrum is a world in itself, and that the study of it will never be completed. His remark is perfectly just. But the spectrum is only a single manifestation of that infinite ether which makes known to us the presence of the universe, and in which whatever exists—if I may be permitted to say so—lives and moves and has its being. What object, then, can be offered to us more worthy of contemplation than the attributes of this intermedium between ourselves and the outer world? Its existence, the modes of motion through it, its transverse vibrations, their creation of the ideas of light and colours in the mind, the interferences of its waves, polarisation, the conception of radiations and their physical and chemical effects—these have occupied the thoughts of men of the highest order. The observational powers of science have been greatly extended through the consequent invention of those grand instruments, the telescope, the microscope, the spectrometer. Through these we have obtained more majestic views of the nature of the universe. Through these we are able to contemplate the structure and genesis of other systems of worlds, and are gathering information as to the chemical constitution and history of the stars. In this noble advancement of science you, through some of your members, have taken no inconspicuous part. It adds impressively to the honour you have this day conferred on me, that your action is the deliberate determination of competent, severe, impartial judges. I cannot adequately express my feelings of gratitude in such a presence publicly pronouncing its approval on what I have done.—I am, gentlemen, very truly yours, John W. Draper."

Microscopy.—In a memoir devoted to the subject of amyloid degeneration of the kidney, liver, and spleen, which appears in a recent part of the "Archives de Physiologie," M. Cornil gives the results of his experiments with several new colouring matters. Two of these were methyl-aniline violets discovered by M. Lauth; the third was a violet discovered by Dr. Hofmann, of Berlin. The preparations can be stained with these violets either when fresh or after being hardened in spirit; and the colouring agents have this peculiarity, that certain tissues, as cartilage, decomposes them into a violet-red and a blue-violet, each of which becomes fixed in different elements of the tissue; the hyaline matrix, for example, assuming a red colour, whilst the nuclei and cellules, as well as the cartilaginous capsules, become of a blue-violet tint. The normal tissues of the liver, kidney, and spleen, however, do not decompose the violets, but, when amyloid degeneration is present, the degenerated and semi-transparent parts resembling colloid become of a violet-red, whilst the normal elements are tinted of a violet-blue, and thus a means equal, if not superior, to that of iodine is afforded, by which the changes may be followed.

The researches of the Rev. W. H. Dallinger and Dr. Drysdale, on monads with high powers, have led to some improvements in the mode of illumination employed in such observations. Great stress is laid upon the necessity of accurate centreing throughout every part of the microscope. The contrivance

used in centreing the condenser consisted of an accurately centred diaphragm beneath the optical portion, having an aperture of not more than the ninetieth or hundredth of an inch: by means of the adjusting screws the image of this aperture is brought carefully to the centre of the field of a moderate power. If the illuminating pencil be now carefully manipulated, and the 1-50th objective be put on, it will be found that the image can be brought to a focus presenting a central disc of light with a margin of the black diaphragm. By an accidental movement of the mirror Mr. Dallinger found that this minute spot of light might be spread over the entire field, presenting a disc, with an intense minute speck in the centre, with an equal diffusion of rays all round, excepting that the illumination grows uniformly less as it reaches the margin of the field. This light was found to be extremely favourable for the display of minute detail under such high powers as the 1-25th and 1-50th. The difficulty in using this mode of illumination was that its production was a matter of great uncertainty. At last it was found that great precision was required in the position of the lamp-flame, which needed as accurate centreing as the test of the apparatus. This was at last effected by means of a lamp with vertical and horizontal screw adjustments, which enabled the narrow edge of the flat flame employed to be accurately brought into position. The lamp is figured in the "Monthly Microscopical Journal" for April.

The following process is recommended by Mr. C. J. Müller for cleaning Foraminiferæ from chalk. Having obtained a quantity of the shells by the usual process of elutriation, mix it with four or five times its bulk of silver sand which has previously been well washed, and put the mixture in a long 2 or 3 ounce phial with a sufficiency of water. Shake up the whole (not violently) for ten or fifteen minutes, and then pour off the turbid water. Renew this operation as many times as you like. The Foraminiferæ will always settle down last, and form a distinct stratum upon the surface of the deposited sand. The sand when shaken up with the shells will act as a gentle rasp, and remove from their surface most of the hard granular particles which injure their appearance. When the cleansing operation is completed the water will rapidly clear upon the mixture being set aside for three or four minutes. There is no difficulty in separating the shells from the sand. Let the whole quietly settle down; pour off the clear water, and allow the whole to rest for a few minutes. Now add a fresh supply of water rather forcibly, when the shells will immediately rise, leaving the sand below. The water with the shells must now be poured off, before they have time to settle down, into another vessel, where they may subside. The deposit may then be dried and mounted as required.

A series of very successful micro-photographs have lately been presented to the Royal Microscopical Society: they are the work of Edward H. Gayer, Professor of Surgery in the Medical College, Calcutta. The camera and other apparatus are arranged on a strong table, 2 feet wide and 12 feet or more long, the chief peculiarity being the facilities afforded for manipulating the mirror, microscope, and illuminating apparatus—a point upon which great stress is laid by Mr. Gayer. To effect this the operator stands near the mirror, which receives the direct rays of the sun through an aperture in a shutter, a cell containing a solution of alum or ammonio-sulphate of copper being used to intercept the heat-rays. This mirror is best made of silvered glass, with the silvered side upwards; the pencil next passes through a large condenser, consisting of a photographic landscape lens of 10 to 12 inches focus; the usual achromatic condenser of the microscope is used if needed. The body of the microscope is connected by a cone with the camera, which is most conveniently made entirely open: when in use it is covered with a black cloth, which is supported by the ends and diaphragms. The focussing screen consists of a sheet of albumenised paper pasted over a glass plate: two diagonal cross-lines should be drawn to assist in centreing, and a word in small type pasted in the middle. The focussing is effected by means of a telescope made of the object lens of a large opera-glass and one of the eye-pieces of the microscope, connected by means of a suitable length of tubing. This contrivance enables the operator

to observe the image on the screen while in a convenient position for adjusting the microscope and illuminators, and also to watch the process during exposure and alter the light if needed. The eye-pieces are not to be used, as they contract the field and blur the image: magnifying power is better obtained by distance. A sufficiently strong eye-piece should be used in the telescope to give a clear view of the minutiæ of the specimen. The hands are the best heliostat, as the mirror is within easy reach the whole time. Instantaneous micro-photography should be attempted with no power higher than ½ an inch, and the large condenser only should be used. The alum cell should be removed during the time of exposure. A special shutter will also be required between the large condenser and the object to be photographed: this shutter should have no connection with the table, as it would communicate a vibration to the whole of the apparatus at a time when everything should be perfectly still, except the movements of the animalcule to be photographed. A good shutter is made with two boards, having round holes cut in them, and made to slide one over the other, an india-rubber band being the motive power. In this way one hole may be made to pass the other with such speed that the exposure is absolutely instantaneous, and the portraiture of rapidly moving subjects rendered perfectly easy with a good light. Some of the results may be seen in the Indian Museum, London. The higher objectives, such as the 1-16th, work better on the immersion principle, and glycerine or oil of cassia may be used instead of water. When substances with a higher refractive index than water are used, the focal distance between the objective and the glass cover will be increased, and the screw-collar must be adjusted to suit the more refractive medium in which the objective has been immersed. When oil of cassia is employed, the screw-collar should be used so as to close the combination completely, and it will then be found quite possible to focus through rather thick covering glass.

MINING.

There are 11,294 Chinese miners in the Colony of Victoria, many of whom know nothing of the English language; in some of the districts they are employed in quartz mines and alluvial mines of great depth and extent, and it has been found necessary to inform them of the provisions of the Regulation of Mines Statute. The Act to provide for the Regulation and Inspection of Mines has therefore been translated into Chinese.

A controversy having arisen in Australia on the priority of discovery of the nickel deposits of New Caledonia, the Rev. W. B. Clarke, geologist to the Colony of New South Wales, recently gave a history of the discovery before the Royal Society of Sydney. He showed plainly that the nickel was first discovered by M. Jules Garnier in his exploring expedition undertaken under the auspices of the French Colonial Office. Mr. Clarke has had in his collection, since 1864, specimens of nickel ore sent him by M. Garnier, who, on his return to France, made known the abundant existence of nickel in the island. Mr. Clarke transmitted an account of the discovery to the celebrated mineralogist Dana, who described this ore of nickel as a new mineral species, in the most recent edition of his well-known work. Prof. Liversidge, of the University of Sydney, also described the new substance in a learned memoir. Clarke, Dana, and Liversidge gave the name of Garnierite to this new ore, in honour of its discoverer. The great rise in the price of nickel has latterly drawn the attention of manufacturers to these deposits. The serpentines, and generally speaking all the rocks which accompany them, are often covered with a fine green coating—a silicate of alumina, nickel, and magnesia. The price of the metal is now 40 francs per kilo., and the demand is still increasing. Hitherto it has been extracted from speiss, in which it occurs combined with sulphur, arsenic, antimony, cobalt, &c. With the ore of New Caledonia the extraction of the metal will be simpler and the product less impure, the nickel being here combined merely with earthy matters. Though of a characteristic green, this new ore may nevertheless be confounded with carbonate of copper; and perhaps the miner, deceived by this resemblance,

may have already met with Garnierite in other countries, and passed it over as a poor ore of copper not worth closer examination. The mines of New Caledonia have already sent to France a ship charged with 500 tons of this mineral.

Some fields of iron ore have been recently discovered in the South of Russia. They are situated partly in the Verchni-Dnieprovsky district of the Ekaterinoslaw Government, and partly in the Elizavetgradsky district of the Cherson Government; iron ore is found here on the rivers Saksagane and Ingouletz, near the village Krivoy-Rog. About 12 miles from this place on the river Saksagane, near the village Tcheivonnaia-Balka, large quantities of red hematite are found. Immense layers of hematite, 100 feet thick, are situated near the river Ingouletz and the village Doubovaia-Balka. Mr. Sergius Kern, of St. Petersburg, says that it is estimated that these new fields contain altogether 90,000,000 tons of ore.

The attention of the Madras Government having been again called, after a lapse of nearly forty-two years, to the occurrence of gold in the Malabar District, it was considered advisable that an examination of the country should be made by the Geological Survey of India. It now, however, turns out that the area over which the auriferous deposits and quartz reefs extend is so large that a considerable period of time must elapse before a full report of the whole district can be made. In the meanwhile, as a gold mining company had been started with the intention of opening up the quartz reefs known to exist in Wynád, and more particularly those near Dayvállah, the attention of the Deputy Superintendent, Mr. William King, B.A., was first directed to this region. The country examined up to this time constitutes a local division of this part of the district. The intermediate elevated terrace of mountain-land lying between the low country of Malabar, the loftier *plateau* of the Nilgiri mountains, and the Mysore territory, called the Wynád, has been separated into North Wynád, South Wynád, and South-east Wynád; and these larger areas are again parcelled out, after a native classification, into *Amshams*. In 1793 the gold mines of Malabar appear to have been noticed by the then Governor of Bombay, who tried to get information on the subject; and they were farmed by the Madras Government in 1803. In 1831 Mr. W. Sheffield, Principal Collector of Malabar, wrote an interesting report on these gold mines, upon which Lieut. Woodly Nicholson was deputed to explore the country with a view to the development of this industry. A committee was then appointed, and the Report, dated May 25, 1833, practically condemned the working for gold, as an European industry, in the low country of Malabar. In 1865 or 1866 Mr. Stern paid a prospecting visit to Wynád, and made trial of the alluvial deposits, of which there are several in the form of flat swampy land along the courses of the streams. Within the last year or so attention was again called to the occurrence of gold in the Wynád. Some of the planters had lived in Australia previous to their coffee experiences, and being more or less acquainted with quartz and its occasional associated minerals they were naturally struck with the quartz in Wynád, while they also knew that gold was and is obtained by the natives. The Alpha Gold Company was then started, the prospectus of which states that the stone will yield about 1 ounce of gold to the ton of quartz. The gold obtained from the reefs is of a pale colour; that from the leaders and washings is generally yellow: and that from the surface washings nearly always of a good yellow colour. Up to the present time Mr. King's observations appear to show that quartz-crushing should be a success. The average proportion of gold for fifteen trials on different reefs is at the rate of 7 pennyweights to the ton, and it is almost certain that many of these would have given a better outturn could more perfect crushing apparatus have been used at the time. The fineness or touch of the ore is inferior to that of Australia, but it compares favourably with Californian reef gold. The percentage of 86·86 is given as a fair average, for on looking at the differences between alluvial and matrix gold in other regions, it is found that they agree very closely. The value of Wynád reef gold, when compared with the mint standard of £3 17s. 10½d., is about Rs. 36-12-2 per ounce troy, which is, of course, somewhat lower than the mercantile rate: 7 pennyweights, or the out-

turn of 1 ton of stone, would then be worth Rs. 12-13-10, which would leave a balance of Rs. 4-2-8 on every ton crushed, even if the high Australian rate were ever attained. Until more is known of the gold-producing powers of the Wynád, no better guidance can be given than the following, by Mr. A. R. C. Selwyn, Director-General Geological Survey of Canada:—"It should not be forgotten that the most favourable indications are not always reliable, and the sanguine prognostications they so frequently give rise to are not borne out by the result of actual working; wherefore I should, even under the most favourable circumstances, not advise any one to invest in such enterprises to an amount beyond what he can afford to lose without serious embarrassment."

GEOLOGY.

Geology.—In a second paper on "The Bone-caves of Cresswell Crags," the Rev. J. Magens Mello, M.A., F.G.S., gives an account of the examination of a chambered cave called Robin Hood's Cave, situated a little lower down the ravine on the same side, The section of the contents of this cave showed a small thickness of dark surface-soil, containing fragments of Roman and Mediæval pottery, a human incisor, and bones of sheep and other recent animals; over a considerable portion a hard limestone breccia, varying in thickness from a few inches to about 3 feet; beneath this a deposit of light-coloured cave-earth, varying in thickness inversely to the breccia, overlying a dark-red sand about 3 feet thick, with patches of laminated red clay near the base, and containing scattered nodules of black oxide of manganese, and some quartzite and other pebbles, which rested upon a bed of lighter-coloured sands containing blocks of limestone, probably forming part of the original floor of the cavern. The hard stalagmitic breccia contained a great many bones, chiefly of small animals, but with some of reindeer, and teeth of *Rhinoceros tichorhinus*, hyæna, horse, water vole, and numerous flint-flakes and chips, and a few cores. Some of the flakes were of superior workmanship. A few quartzite implements were also found in the breccia. The cave-earth contained a few flint implements, but most of the human relics found in it were of quartzite, and of decidedly palæolithic aspect. There was also an implement of clay-ironstone. The animal remains chiefly found in the cave-earth were teeth of horse, *Rhinoceros tichorhinus*, and hyæna, and fragments of both jaws of the last-mentioned animal. Bones and teeth of reindeer, and teeth of cave-lion and bear also occurred. The red sand underlying the cave-earth contained but few bones, except in one place, where antlers and bones of reindeer and bones of bison and hyæna occurred. At another part a small molar of *Elephas primigenius* was found. A large proportion of the bones had been gnawed by hyænas, to whose agency the author ascribed the presence of most of the animal remains found; but he remarked that no coprolites of hyænas had been met with. Prof. W. Boyd Dawkins has drawn the following conclusions, as to the history of Robin Hood's Cave, from the results of Mr. Mello's researches. He considers that the cave was occupied by hyænas during the formation of the lowest and middle deposits, and that the great majority of the other animals whose remains occur in the cave were dragged into it by hyænas. That they served as food for the latter is shown by the condition of many of the bones. During this period the red sand and clay of the lowest stratum was deposited by occasional floods. The red loam or cave-earth forming the middle stratum was probably introduced during heavy rains. The occupation of the cave by hyænas still continued, but it was disturbed by the visits of Palæolithic hunters. The remains found in the breccia indicate that the cave was inhabited by man, and less frequently visited by hyænas than before. The presence of vertebræ of the hare in the breccia would imply that the hunters who occupied the cave had not the dog as a domestic animal. The traces of man found in the cave consist of fragments of charcoal and implements made of antler and mammoth tooth, quartzite, ironstone, greenstone, and flint. The distribution of these implements in the cave represents three distinct stages. In the cave-earth the existence of man is indicated by the quartzite implements, which are far ruder than those generally formed of the more easily fashioned flint. Out of 94 worked

quartzite pebbles only 3 occurred in the breccia, while of 267 worked flints only 8 were met with in the cave-earth. The ruder implements were thus evidently the older, corresponding in general form with those assigned by De Mortillet to "the age of Moustier and St. Acheul," represented in England by the ruder implements of the lower breccia in Kent's Hole. The newer or flint series includes some highly finished implements, such as are referred by De Mortillet to "the age of Solutré," and are found in England in the cave-earth of Kent's Hole and Wookey Hole. The discovery of these implements considerably extends the range of the Palæolithic hunters to the north and west, and at the same time establishes a direct relation in point of time between the ruder types of implements below and the more highly finished ones above.

The mode of occurrence of phosphatic deposits in various localities in Canada has been described to the Geological Society by Principal Dawson, LL.D., F.R.S., &c. Dark phosphatic nodules, containing fragments of *Lingulæ*, abound in the Chazy formation at Allumette Island, Grenville, Hawkesbury, and Lochiel. Similar nodules occur in the Graptolite shales of the Quebec group at Point Levis; and in limestones and conglomerates of the Lower Potsdam at Riviere Ouelle, Kamouraska, and elsewhere on the lower St. Lawrence; these deposits also contain small phosphatic tubes resembling *Serpulites*. The Acadian or Menevian group near St. John, New Brunswick, contains layers of calcareous sandstone blackened with phosphatic matter, consisting of shells and fragments of *Lingulæ*. The author described the general character of the phosphatic nodules examined by him at Kamouraska, and gave the results of analyses made of others from various localities, which furnished from 36·38 to 55·65 per cent of phosphate of lime. A tube from Riviere Ouelle gave 67·53 per cent. The author accepted Dr. Hunt's view of the coprolitic nature of the nodules, and inclined to extend this interpretation to the tubes. The animals producing the coprolites could not be thought to be vegetable feeders; and he remarked that the animals inhabiting the primordial seas employed phosphate of lime in the formation of their hard parts, as had been shown to be the case with *Lingulæ*, *Conularia*, and the Crustaceans. The shells of genus *Hyolithes* also contain a considerable portion of phosphate of lime. Hence the carnivorous animals of the Cambrian seas would probably produce phosphatic coprolites. With regard to the Laurentian apatite deposits, the author stated that they, to a great extent, form beds interstratified with the other members of the series, chiefly in the upper part of the Lower Laurentian above the *Eozoon*-limestones. The mineral often forms compact beds with little foreign matter, sometimes several feet thick, but varying in this respect. Thin layers of apatite sometimes occur in the lines of bedding of the rock. Occasionally disseminated crystals are found throughout thick beds of limestone, and even in beds of magnetite. The veins of apatite are found in irregular fissures; and as they are found principally in the same parts of the seams which contain the beds, the author regarded them as of secondary origin. The Laurentian apatite presents a perfectly crystalline texture, and the containing strata are highly metamorphosed. The author's argument in favour of its organic origin are derived from the supposed organic origin of the iron ores of the Laurentian, from the existence of *Eozoon*, from the want of organic structure in the Silurian deposit described by Mr. D. C. Davies, and the presence of associated graphite in both cases, from the character of the Acadian linguliferous sandstone, which might by metamorphism furnish a pyroxenite rock with masses of apatite, like those of the Laurentian series, and from the prevalence of animals with phosphatic crusts in the Primordial age, and the probability that this occurred also in the earlier Laurentian. The position of the phosphatic deposits above the horizon of *Eozoon* is also adduced by the author as adding probability to the existence of organic agencies at the time of their formation.

At a meeting of the Manchester Geological Society, Mr. Joseph Dickinson, F.G.S., said that recently a member of the Society (Mr. Binney) enquired of him whether, underneath the coal seams of Kilkenny and Queen's County (Leinster coal-field) any trace was found of the *Stigmaria ficoides*—

that was, the rootlets—or whether the means by which the coal had been converted into anthracite had destroyed them. As the question might be interesting to other members, he exhibited some of the fire-clay which underlay these coal seams, showing that the rootlets of the *Stigmaria ficoides* were as plainly to be seen in the floor of some of the seams as in our English seams. Professor Dawkins had observed in the under clay of Pembrokeshire—which was, he supposed, of the same relative antiquity as that of Leinster—rootlets of *stigmaria*; but anything like one of the specimens shown by Mr. Dickinson (a granite-like rock) he had never seen. In that specimen felspar, mica, and quartz were perfectly shown. A specimen of crushed clay, which was exhibited, was very interesting, as showing the enormous pressure to which it had been subjected. Mr. Dickinson explained that the granite-like specimen was taken from underneath the fire-clay that lies below the deep coal seam at Clogh Colliery, Castlecomer.

At the same meeting of this Society, Professor Boyd Dawkins said that, when in New South Wales, he had an opportunity of critically examining the coal-fields of that colony, and there could be no doubt as to its geological age: for, in its lower portion, there were marine fossils, very much after the same fashion as we have in our own coal-field of Lancashire. Above this, in the coal measures proper, there are *lepidodendron*, *calamites*, and *sigillaria*, associated with some peculiar ferns known as *Glossopteris*. Up to the present time the age of that coal-field had been in dispute between the Rev. W. G. Clarke on the one hand, and Prof. McCoy on the other; and it appeared that Prof. McCoy had only argued from the fern alluded to, which occurs in the lower mesozoic strata of India. But, certainly, when we take into consideration the marine fauna—the shells living in the sea of the lower carboniferous strata—and the terrestrial vegetation, which was to a large extent identical with that in our own coal-field, there was not much doubt as the New South Wales coal being Palæozoic.

MINERALOGY.

M. D. Loiseau has discovered a new crystalline organic compound, to which he gives the name of raffinose. Its elementary composition is—Carbon, 36·30; hydrogen, 7·07; oxygen, 56·63; corresponding to the formula $C_6H_7O_7$, or to one of its multiples. It is almost devoid of sweetness; its rotatory power when dissolved in water is greater than that of sugar.

M. Domeyko publishes analyses of two new meteorites from the Desert of Atacama: (1) Meteoric iron from Cachiyuyal. This meteorite weighed 2·55 kilos. On analysis it yielded—Iron, 93·72; nickel, 4·81; cobalt, 0·39; schreibersite, 0·40; earthy matter, 0·50. (2) Meteoric iron from Mejillones. Its composition is—Iron, 95·4; nickel, 3·8; cobalt, 0·1; schreibersite, 0·9. To the same gentleman we are indebted for an analysis of a new mineral named daubreite. This mineral is an earthy mass of a yellowish or greyish white, containing a great number of crystalline lamellæ, opaque, and of a nacreous lustre. Its hardness does not exceed 2 to 2·5, and its specific gravity is 6·4 to 6·5. Its composition is—Sesquioxide of bismuth, 72·60; sesquichloride of bismuth, 22·52; water, 3·84; sesquichloride of iron, 0·72.

There is a rock intervening between the Gneiss Rocks of Mantiqueiro which approximates to epidote, and appears to consist of a single mineral, with the exception of some small granules of infusible quartz. Its specific gravity, according to M. H. Gorceix, is 3·4; it fuses easily, leaving a black slag, the specific gravity of which is only 2·86; its hardness is between 6 and 7. The composition of this rock is—Silica, 38·5; alumina, 25·1; lime, 23·2; protoxide of iron, 10·4; magnesia, traces; loss on ignition, 2·6.

M. A. Damour has examined a calcareous alabaster from Mexico. This material, known in commerce as the onyx of Tecali, varies in colour from milk-white, yellowish white, to pale green, certain samples displaying brown veins shading into red. It takes a fine polish. Its specific gravity is 2·77. It is readily and entirely soluble in nitric acid. Its composition is—Carbonic acid, 43·52; lime, 50·10; magnesia, 1·40; ferrous oxide, 4·10; manganous oxide, 0·22; water, 0·60; silica, traces.

The optical and crystallographical properties and chemical composition of microcline, a new species of triclinic felspar with a potassic base, have been described by M. des Cloizeaux. The composition of this mineral is—Silica, 64·30; alumina, 19·70; ferric oxide, 0·74; potash, 15·60; soda, 0·48; loss on ignition, 0·35. The specific gravity is 2·54.

TECHNOLOGY.

Dr. H. D. Meidinger contributes a valuable paper, on "Progress in the Artificial Production of Cold and Ice," to Dr. A. W. Hofmann's "Report on the Development of the Chemical Arts during the last Ten Years." Speaking of the preservation of ice, he says that ice machines, however they may be eventually improved and their effect increased, will never, in the more northern parts of the temperate zone, where a moderately cold winter with frost is generally experienced, acquire importance enough to meet the demand even approximately. They will serve merely as valuable substitutes to render us independent of the fickleness of the seasons. Even in more southern regions, where ice machines are the only source for obtaining ice, they must work to stock and fill magazines, since the demand does not go hand in hand with the production, but varies with the weather. There is in general no conception of the quantities of ice which certain trades require, and which are consumed in domestic life where its use has grown into a necessity. In 1866 the quantity of ice consumed in New York and its vicinity amounted to 250,000 tons (254,015 metric tons), or 5 cwts per head. The weight stored up was 543,000 tons (551,721 metric tons, whilst the capital employed in the trade amounted to 2,160,000 dollars. The retail price was for quantities of 5 to 12 kilos. 4 pfennige (the German "pfennig" is about the tenth part of an English penny) per kilo., but for quantities of 1 to 10 cwts. only one shilling per cwt. In 1871 a company in Berlin, the "North German Ice Works," stored up 600,000 cwts. of ice, and delivered it to subscribers at 77 pfennige per cwt. The quantities of ice consumed in brewing may be learned from the following data, which the author obtained in 1869 from Dreher's brewery at Klein Schwechat, near Vienna:—This establishment brewed, in 1867, 483,150 Viennese eimers, = 273,463 hectolitres, and stored up 515,600 cwts. (28,874,219 kilos) of ice. In the following year these numbers rose to 492,499 eimers (278,754 hectolitres) of beer and 563,058 cwts. (31,531,924 kilos. of ice. On an average 1 cwt. of ice is used per eimer (56.6 litre). In a prolonged frost of 2 months this quantity can be procured at the cost of 7 Austrian kreutzers (14 pfennige) per cwt. In shorter periods of cold the price rises to from 10 to 12 kreutzers, to which must be added 1 kreutzer for shovelling into the ice cellars. In mild winters the ice is brought in part from Styria; as the cold weather in 1869 set in late, 26,000 cwts. (1,456,031 kilos.) were procured from there, costing, by the time it reached the brewery, 115 florins per 200 cwts.

Schweinfurt-green, known also as mitis-green and mountain-green, is a compound of the arsenite and acetate of copper too frequently used, in spite both of advice and of legal prohibition, for colouring paper-hangings, ball-dresses, artificial flowers, wafers, bon-bons, and toys. Every winter we hear of needlewomen experiencing serious affections after having made up costumes of tarletan coloured or printed with Schweinfurt-green; that dancers wearing similar materials, and with wreaths of artificial flowers in their hair, experience violent headache on the morning after the ball; that similar symptoms have been experienced by persons who have slept for some nights in rooms papered or painted with green; and, lastly, that children have been seized with vomiting after having eaten bon-bons or sucked toys coloured green. M. Kupfferschlæger recommends the following method of detection:—The stained paper, divided into strips, is placed in a porcelain soup-plate, and covered with a solution of chlorate of potash saturated when hot, and the whole is heated in the water-bath till the paper is completely dry. Then it is set on fire, and instantly covered with a large glass bell so that nothing may be lost. The ash is pulverised, and immediately exhausted in the cold with the water which has served to rinse out the

bell and the plate after the combustion. Thus all the arsenic combined with the potash is dissolved, and not the oxides of chrome, copper, aluminium, and of tin and lead if present. This colourless solution, filtered, and mixed with sulphuric acid till a slight acid reaction is distinguished, is then introduced into a Marsh's apparatus which has been submitted to a blank test, and found free from arsenic.

It was observed by Kœchlin that woollen cloth dyed blue with indigo was notably decolourised by being allowed to freeze. This result, according to the experiments of Goppelsrœder, was due to ozone present in the air, which acts even at temperatures below 0°, but only when the tissue is wet. Cochineal reds on wool were decidedly impoverished by exposure to the action of ozone for eight days, but were not discharged. Aniline-black was unchanged; aniline-brown on cotton was turned an orange-yellow; magenta, aniline-blues and violets, coralline, and iodine green were discharged, as were also lakes of the dye-woods, and even Turkey-red. Ozone is of great importance in the development of certain colours. Aniline-black, made up with hydrochlorate of aniline, sal-ammoniac, thickening, sulphide of copper, and chlorate of potash, was developed by the aid of ozone in 1 to 1½ hours. For the development of ozone on the large scale Goppelsrœder proposes Gramme's machine, in which the electric spark passes through a conductor repeatedly interrupted. The injurious effects of frost upon alizarin paste, alumina, gelatin, cochineal lakes, mordanted tissues, &c., are well-known, but by no means thoroughly understood.

MM. E. Fremy and PP. Dehérain, in their researches on the sugar beet, show that saline solutions identical in composition act very differently upon beets accordingly as the roots plunge into the solutions themselves, or as the latter merely occupy the pores of the soil. On planting beets of different origin in identical conditions as to soil, manure, and watering, roots are obtained differing in their yield of sugar. An excess of nitrogenous manure lowers the percentage of sugar in all beets, but those of a superior strain preserve still such a quantity of sugar that they may be advantageously treated. To produce from a given surface the maximum of sugar under conditions advantageous alike for grower and manufacturer, we must depend above all on a judicious selection of the seed.

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I. JAPANESE MINES.

IT is certain that in almost every portion of the empire of Japan minerals of various kinds are found, and it is equally certain that in numberless parts of the country mines have been worked, with more or less success, for centuries. So says the Hon. F. R. Plunkett in a recent Report to our Foreign Office, and under his guidance we will endeavour to see how, unaided by the means and appliances of modern science, that strange and long-isolated race —with whose ways we are only now, in the last quarter of the nineteenth century, gradually becoming somewhat more familiar—contrived, during her ages of seclusion, to make the earth give up her mineral wealth. The interior of Japan and its resources are still so little known to foreigners that Mr. Plunkett, even with the advantages of his official position, could hardly have hoped to achieve any very satisfactory result in the course of his investigations, had not the Japanese Government placed at his disposal the valuable information in their possession, and had not their chief mining engineer, as well as the metallurgist of the Imperial Mint at Osaka, rendered him most efficient aid in the prosecution of his researches. With such assistance, however, and by careful observation on his own part, he has collected a mass of useful statistics, as well as much curious and interesting information.

With one solitary exception, where foreign skill has been availed of with good results, the mines of Japan are still attacked exclusively by adits, for the natives never sink a shaft, and, as they have nothing more powerful than bamboo pumps for lifting water, it is easily conceivable how soon

it gets the upper hand, and how the number of abandoned mines comes to be so large.

Coal-mines occupy the most important place among the mining industries of Japan, and the best developed and most productive of these is undoubtedly in the small island of Takashima, some 10 miles from Nagasaki. Up to 1868 this mine produced but a small quantity of coal, owing to the defective system of mining pursued by the natives, viz., working the seam from its highest level, where the coal strata crop up to the surface, and following it downwards in the direction of its dip. Since that time, however, the mine has been worked under foreign superintendence, the result being a vast improvement in the amount and quality of the output. The district of Karatsu, to the north of Nagasaki, contains several coal-mines, little inferior to the foregoing, which are still worked in the native manner, and of one of which we will give a brief description. The road to these mines runs inland to a distance of nearly 9 miles from the town, passing up a valley of singular beauty and richness, from 1 to 3 miles wide, hemmed in by mountains, most of which are clothed with luxuriant forests to their very summits: the lowland is all well cultivated, and produces rice and cotton; while along the foot of the mountains wheat, millet, beans, and the paper-tree grow in great quantities. The river which runs through this valley, nearly parallel to the road, is a regular mountain stream, with a broad bed and little depth of water, but it is just sufficient to float the small flat-bottomed boats carrying the coal down to the sea. The mine, visited by Mr. Plunkett, has but one entrance, viz., an adit bored through the rock, about 4 feet wide and $3\frac{1}{2}$ feet high, carried down at an angle of 56° until it reaches the seam of coal about 100 yards from the entrance. The mode of working the coal and getting it up to the surface is very peculiar. The miners work lying on their sides or backs, and never can stand up. The coal is removed with small picks and wedges driven in with a hammer; it is then placed on small oblong bamboo baskets, each holding rather more than 1 cwt., provided with iron runners, which just fit the wooden sides of the ladder, laid along the floor of the entrance adit. This forms a kind of rude tramway, by which the baskets are hauled up to the surface. They are drawn by boys, apparently not more than 12 or 14 years old, who crawl on all-fours, harnessed to the basket by a very short yoke,—so short, indeed, that even when out of the mine, and dragging their loads across to the coal-yard, they are still obliged to crouch in the same painful manner.

They haul their loads up the incline, holding with their hands and feet to the ladder before mentioned. The mine is lighted with common open oil-lamps, and no precautions are taken against fire-damp (which, owing to surface coal only being worked, does not seem to be troublesome in Japanese mines); its ventilation below is described as atrocious, and the means for getting rid of water are primitive in the extreme, so much so that Mr. Plunkett's account of them deserves to be quoted. The working galleries being only about 70 feet below the surface, just above these a large square well is dug to a depth of about 50 feet, and the water from below is pumped into this by small hand bamboo pumps, capable of lifting about as much water as an ordinary garden syringe. On the surface, close to one side of this well, stands a high post, on which works a long walking beam, at one end of which, fastened to a thick bamboo, is a large tub; the bamboo is long enough to allow the tub to reach the bottom of the well; at the other end of the walking beam are attached four ropes. Inside the well, about 10 feet down, is a small platform, on which a man stands to receive the tub as it comes up. Suppose the tub to be at the bottom of the well, four men, holding the ropes at the end of the walking beam, run down the side of a small mound, thereby lifting the tub to the level of the platform, where the man, seizing it, turns the water over into a small channel made to carry it out into the valley, which is a few feet below, and where there is a small stream communicating with the river further down. The four men then walk up the hill again, thereby letting the tub fall to the bottom of the well to be refilled, and so on till the water is reduced to a proper level. The coal is taken from the pit's mouth to the river-side in small carts, which merit a few words in passing. They are of two kinds; one being a bamboo basket placed on low wheels, and fitted at each end with semicircular shafts bending downwards, so that the hind shafts can be made to scrape along the ground, and thus act as a drag; and the other a narrow wooden box, mounted on two high wheels, and fitted with a pair of shafts about 7 feet long, joined together at the end. Both these conveyances carry from 2 to 3 cwts. of coal, and are equally clumsy; the only difference between them is that, whereas the baskets are dragged, the boxes are pushed, and, as a rule, men are employed to work the latter and women the former.

Copper, which ranks next in importance among the minerals of Japan, is found in numberless parts of the country,

and is usually of excellent quality. When properly refined it takes a foremost place amongst the various kinds of commercial copper, and it is almost invariably free from the injurious metals, such as antimony and arsenic. A brief account of a copper-mine in Yamato, in the neighbourhood of Osaka, will probably possess some interest, inasmuch as from various circumstances it is doubtful, in the opinion of the metallurgist of the Japanese Mint, whether European processes would profitably and successfully replace the native methods at present followed. This mine is situate in a narrow valley in the midst of mountains, and to reach it two passes, nearly 3000 feet high, have to be crossed. So narrow, indeed, is this valley, and so steep the inclination of its sides, that there is no level ground, nor is there ground which could be levelled, within nearly half a mile of the entrance of the mine, and all the present dressing-houses, furnaces, &c., are perched upon the slopes, and supported by built-up foundations. The levels in this mine are driven and the ore removed by the aid of gunpowder, and the pieces broken down are carried to the surface on the backs of men and women. The ore, having been broken up into pieces about the size of walnuts, is placed, with a sufficiency of wood, in kilns 5 feet square, with walls rudely built of stone and lined with clay. The calcination lasts fifteen days, but is of a very unsatisfactory character, much of the ore undergoing but slight change in the process. The furnace consists of a hemispherical cavity in the ground, lined with refractory clay: it is furnished with two bellows at the back and one in front, and is half covered with a thick semicircular lid of clay. The process of melting is thus described by our authority:—The furnace, having been repaired and well dried after the "heat" of the previous day, is lighted by introducing some burning embers, together with fresh charcoal, and over this charcoal a third part of the charge of ore is piled, forming a heap; the blast from the bellows behind is started, and soon jets of flame of carbonic oxide make their appearance over the whole surface of the heap, which, gradually melting, subsides into the furnace; the slag is then removed, and another portion of charcoal and of ore piled up as before. When the whole of the charge (about 11 cwts.) has been melted down in this way, the slag is skimmed off; after which the front bellows is brought into action, the blast of air being directed upon the surface of the molten "regulus" and copper, and a little charcoal added at the side from time to time to keep up the heat. In ten hours the charge is worked off, and the

copper lifted out, in more or less circular cakes, by throwing water on its surface and raising the solidified crust with an iron tool. This process has many advantages for a country like Japan, where the means of intercommunication are so bad, and coal is, therefore, not available as fuel.

Japan possesses both gold and silver, although not in very large quantities. Mr. Plunkett gives no account of these mines in his Report; in fact he does not believe that they are at present of much value. As the Japanese Government has recently turned its attention to their development, and as considerable interest attaches to the subject, we will condense into as brief a space as possible—from the columns of the "Hiogo News," an English newspaper published in Japan—an account of the silver mines of Ikouno, in Tajima. On the hill-side, above the works, are to be seen a number of places like large rabbit-holes, and a tramway which runs round the face of the hill, connecting the holes with a series of shoots, by means of which the ore is passed down to the works. Entering one of the holes referred to, which prove to be the mouths of galleries, we see the first process of removing the ore by blasting, the fuses for which are now all made on the premises. Emerging, we find the ore at the deliveries of the shoots being broken with hammers into pieces, varying in sizes, the richest portions being broken the smallest. The poorer lumps, and those which contain a preponderance of other minerals, are set aside for consumption at convenience. The more choice morsels are set out, according to quality, in five classes of an estimated value, on appearance of bearing silver in the proportion of from 80 dollars per ton to 5000 and upwards. These fragments are then pounded into dust in crushing-mills, and the dust baked in ovens with common salt. Hitherto the silver has been combined with sulphur, but in these ovens a chemical change takes place, the chlorine of the common salt combining with the silver (and the 10 to 12 per cent of gold which the ore contains), and the sulphur combining with the sodium of the salt to make Glauber's salt, which is sent into the river. The ore—now a red earth—is next, by means of water and iron balls, thoroughly mixed with a large quantity of quicksilver, by the aid of revolving drums. Under this process the quicksilver takes up the precious metal, and when the amalgamation is complete the drums are emptied, and the now comparatively valueless mud washed away. The combined, and still fluid, metals are then treated with hydraulic pressure against a leather sieve, through which free mercury is extruded, leaving a putty-like, brilliant, white amalgam. Heated in iron retorts, the remaining mercury

contained in this amalgam is driven off into a condenser, to be used over and over again. The resulting lumps of metal having been fused with borax, which brings away some scoriæ and other impurities in the form of scum, are run into moulds, and sent to the Mint at Osaka. The metal contains in this form, in which it leaves Ikouno, about 70 per cent of pure silver and 10 of gold, the remaining 20 per cent being nearly pure copper. When M. Coignet, the French superintendent of the Ikouno mines, was first appointed, the Government for a long time hesitated to admit that, even with the inferior appliances at his command, he could produce 4000 dols. a month of silver, but being once convinced they expended 400,000 dols. on European machinery, which arrived some three years ago, and, by the aid of a foundry and machine shop on the premises, supplemented by the efforts of the Yokoska Arsenal and the Kobe Iron Works, was soon got into working order. By last accounts the out-turn averaged 30,000 dols. a month, half of which was required for working expenses, leaving the Government a very handsome profit. We are informed that the power utilised to drive the machinery is mainly water, brought 4½ miles in an artificial canal, which is available for nine months out of twelve: during the rest of the year the works are driven by steam, for which purpose there are four engines of 25 horse-power each. In addition to the silver, which is at present the principal object sought, the hills contain copper, iron, lead, and zinc, the first of which it is in contemplation to work independently.

Iron is found in many parts of the country, and the total production thereof ranks next to coal in quantity, though not in value. The following is a brief account, given by Dr. Geerts, of Nagasaki, of the mode adopted by the natives for smelting the iron ore :—After the ore has been selected it is piled up in heaps with coal, and calcined (roasted) in order to expel the water, carbonic acid, sulphur, &c. This calcination makes the ore more porous, and better fitted for the smelting process. The calcined ore is smelted in a cylindrical furnace, built up with a few hard stones and fire-proof clay. The clay is laid in layers till the wall of the furnace has sufficient thickness. The thick bottom of this small furnace has a rounded shape, and a little above the bottom two exactly opposite openings in the wall are made for receiving the tubes of the bellows. Besides, there is a third opening near the bottom, which is closed with a clay stopper, and afterwards is opened to collect the fluid metal in the "forms." Now the furnace,

previously perfectly dried, is filled with a mixture of coarse calcined powdered ore, charcoal, and some felspar, clay, or another quartz containing stone. The latter substances are added to act as a flux, and to separate the metallic iron from the impurities which are taken by the slag : sometimes, but not generally, coal or coke is used as fuel. When the heat produced by the continuous strong stream of air, pressed into the furnace by means of large Chinese bellows worked by four or five men, has been sufficient to smelt the ore, the iron will gradually run in a liquid state to the bottom of the furnace, and is cast in sand forms by removing the clay stopper of the lowest opening. The cold metal is sometimes purified by a second smelting, in another similar but smaller furnace.

Lead is found in many of the provinces, but, owing to the defective way in which the mines are worked, the annual production is but small. The following is, in brief, a description of a lead-mine near the famous Lake Biwa,—at no great distance from Kioto, the ancient capital of the Mikados,—and of the mode of smelting still practised there. The mine is worked by levels driven into the side of the hill, and the water is got rid of by an adit. The levels are driven entirely at random, and though there are native plans of the mine, they give no accurate information, either as to the direction or extent of the workings. Wood is used for fuel in the calcining kilns, and charcoal in the smelting furnaces. The charcoal is that of oak, maple, &c., for although the cryptomeria is the most abundant tree, charcoal made from it is disliked, because it is soft and swift-burning. The calcining kilns measure 4 feet every way, and are open at the top ; their sides are of clay, and usually four, six, or even more, are built side by side in a row. At the bottom of the front of each there is a hole, which is opened or closed when necessary, according to the rate of combustion. The ore, having been previously broken up into small pieces, is placed in these in alternate layers with wood, and undergoes a first calcination for five days : the process is afterwards repeated in other kilns. The smelting furnace is situated under a rude chimney or hood of wicker-work, plastered with clay, open in the front to about the height of 6 feet from the ground, while its back wall screens the men who work the bellows from the heat of the furnace. The furnace is almost the same as that spoken of in connection with copper-smelting. Below the lid, at the back of the furnace, are two holes, by which the blast is admitted, it being conveyed thither by clay tubes running just below the surface

of the ground. The daily charge of a furnace is 600 lbs. of calcined ore, divided into four equal parts; one part being charged into the furnace every hour and a half, and the whole worked off in six hours. The furnace having been lighted by introducing a small quantity of ignited charcoal, a quantity of charcoal—a little less in volume than the 150 lbs. of ore—is filled in; on the top of this the calcined ore is spread, covering it entirely and uniformly, and the heap is then gently patted with a small flat iron tool, and the bellows started gently. The remainder of the process is so much like that already spoken of in the case of copper smelting that we need pursue the description no further.

Besides the mineral productions already alluded to, tin, quicksilver, sulphur, and coal-oil are found in various parts of the Empire of the Rising Sun, but the mines, &c., require development by foreign aid before they can attain any considerable importance.

In conclusion, we may remark that Mr. Plunkett is not very hopeful as to the future prospects of mining in Japan, and he thinks it extremely doubtful whether there are many mines of such position, percentage, and character as would justify the investment of much capital in mining enterprise at present: he, further, expresses a belief that the mineral wealth of Japan has been hitherto estimated by the public far beyond its real value. In a recent article on the subject, the journal which has supplied us with particulars respecting the Ikouno mines ventures to entertain a more sanguine view of the matter, and states that "the more familiar acquaintance with the national resources, which we are always acquiring, satisfies us that on her store of minerals Japan must depend for her future prosperity." "It may be," the writer adds, "that we shall live to see a large export mineral business done, notwithstanding the obstruction which local and personal interest offers to its development; but whether we do or not, we shall not cease to believe that Japan will have to depend upon her mineral wealth for the place in the future she will take among other nations."

II. THE CRADLE OF CIVILISATION.*

THINKERS of no mean standing† have insisted strongly on the fact, real or supposed, that no nation claims civilisation as an indigenous product, but that all speak of it as having been originally introduced from abroad. Hence they infer that its source must have been a revelation communicated to man by some Divine, or at least superhuman, agent. With all deference to such high authorities, we submit that the premisses, even if indisputable, do not by any means justify the conclusion. Were civilisation the result of supernatural instruction, this must still have been imparted to men in some country or countries. Those men would then hold, as regards their neighbours, the very same position as if the lessons they had to communicate were the fruits of their own inventive genius, and the country where the supposed revelation had taken place would be regarded by the rest of the world as the fountain-head of civilisation, and would doubtless claim for itself this proud distinction. According to the supernatural, as well as to the natural theory of the origin of culture, it must be indigenous in some one country, if not in more, and the traditions which everywhere ascribe it an extraneous source can be regarded merely as a proof of the unreliability of early history. Indeed these old legends are in their nature subjective rather than objective, and have their roots in a primitive tendency of the human mind which might be named "elsewhereism" or "alibism." Barbarians, young persons, and the imperfectly educated invariably conceive everything great and wonderful as coming from afar. In their own country, wherever that may be, all is tame, mean, and common-place. But in the dim distance, "where the rainbow touches the ground," lie hidden treasures. In some remote isle flows the fountain of eternal youth. Across the seas, beyond the mountains, at the head-waters, or at the mouth of the great rivers, wealth, fame, wisdom, might be easily won. This feeling is often the cause of emigration. Men believe that they may easily find "elsewhere" the success which has escaped them at home. If their new home proves no more propitious than the old one,

* The Oera Linda Book, from a Manuscript of the Thirteenth Century. The original Frisian text, accompanied by an English version of Dr. OTTEMA'S Dutch translation, by W. I. SANDBACH. London: Trübner and Co.

† As, for instance, Archbishop Whately.

they merely conclude that it is not the right "elsewhere." Had they but gone to Canada instead of to South Africa all would have been well with them.

As with wealth, so with knowledge: "he has travelled for his wisdom" is still a common expression. We have heard the discoveries of such men as Humboldt, Agassiz, Darwin, and Wallace, ascribed not to their genius or their industry, but simply to the fact that they had visited countries remote. Any man who had been where they went might, it is asserted, have done as much. If such notions are still current in our days, need we wonder that in more primitive times all valuable truths were supposed to have been imported from abroad? A philosopher had travelled, and on his return imparted some valuable lesson to his countrymen; forthwith the *post hoc* was converted into the *propter hoc*, and the story ran that he had learnt all this elsewhere; yet the very legends betray themselves. Mr. G. H. Lewes, if we remember rightly, points out how a Greek sage is represented as having studied mathematics under the Egyptian priests, and yet at the same time as having taught them how to measure the height of the pyramids by their shadows! Surely they who required such an elementary lesson would have nothing very valuable to communicate. Nay, even if an originator had not travelled, his countrymen, rather than give honour to a prophet who had sprung up in their midst, would invent some legend of distant travel. This view, which no close observer of human nature can dispute, throws a flood of light upon all the Manco Capacs and other mysterious strangers who suddenly appear among savage tribes, and who, instead of being eaten, are at once accepted as high-priests, legislators, and kings.

We do not, of course, mean to deny that primitive nations learnt much from abroad, and that travellers from time to time brought back valuable lessons. When the means of communication between countries were scanty, philosophers had to travel in person for knowledge, just like merchants for commodities. We seek merely to show the true origin of the misleading sagas which ascribe all improvement to an unknown source, which, like the mirage, vanishes as we pursue.

However, if we may believe certain enthusiastic partisans, the true cradle of our modern civilisation has been at last discovered. The favoured spot is certainly not one which would be guessed by any unprepared reader. It is none of the regions which the researches of historians, antiquarians,

and philologists have indicated as probable. It is not in Egypt or among the "blameless" Ethiopians; not in Assyria or Medea, India or China. We must not seek it in the ruins of Central America, or in that yet more mysterious "City of the Morning Star" whose palaces and temples extend for many a league amidst the forests of Siam. It is in that Dutch province known as Friesland!

It appears that a certain C. over de Linden, Chief Superintendent of the Royal Dutch Dockyard at the Helder, is in possession of a very ancient manuscript, which has been preserved in his family from time immemorial, its origin and contents being totally unknown, though a tradition had been handed down requesting its careful preservation. The manuscript is said to have been left to C. over de Linden by his grandfather, De Heer Andries over de Linden, who formerly lived at Enkhuizen, and died there, in the year 1820, at the age of 61. As the present proprietor was then a child, the precious document was preserved by his aunt, Aafje Meylhoff, born Over de Linden, and living at Enkhuizen, who, in the year 1848, considered that he had reached a sufficiently discreet age to take the family treasure into his own custody.

A rumour concerning the manuscript reached a certain Dr. E. Verwijs, who received permission to examine and copy it, and "was of opinion that it might be of great importance, provided that it was not suppositious;"—a very judicious reservation. However, his copy soon after fell into the hands of a Dr. Ottema, a less critical or a more excitable inquirer, who very soon succeeded in convincing himself of the great age and of the authenticity of the document. His method of reasoning may be gathered from the following paragraph:—

"In old writings the ink is very black or brown; but while there has been more writing since the thirteenth century, the colour of the ink is often grey or yellowish, and sometimes quite pale, showing that it contains iron. All this affords convincing proof that the manuscript before us belongs to the middle of the thirteenth century, written with clear black letters between fine lines carefully traced with lead. The colour of the ink shows decidedly that it does not contain iron. By these evidences the date given, 1256, is satisfactorily proved, and it is impossible to assign any later date. Therefore all suspicion of modern deception vanishes."

Dr. Ottema would perhaps be surprised to learn that these evidences amount absolutely to nothing. He does not appear

to have subjected the writing to any chemical test so as to prove the absence of iron. He assumes the writing to be very old, and then infers, from its deep blackness, the absence of iron! But even supposing Dr. Ottema's conjecture to be correct, and that the ink is free from iron, this proves nothing. "Fine lines carefully traced with lead" can be produced in this degenerate nineteenth century quite as well as in the thirteenth. As a contemporary remarks, "A modern scribe is under no legal or moral obligation to put iron in his ink,"—more especially if his object is the not very moral one of literary forgery. There is therefore nothing in the ink or the characters which can speak in favour of 1256 rather than 1847 as the date of its composition.

The work professes to give fragments of the early history of the Frisians from the date of 2193 B.C. down to the times of Alexander the Great and his immediate successors. The author of the latter part of the book is assumed to have been a contemporary of Julius Cæsar, whilst the first portion, written by Adela, extends backwards to 558 B.C. Here the critical—not to say the sceptical—reader will be at once struck with a new difficulty. Admitting the manuscript to be authentic, and granting that its successive portions were first committed to writing at the dates just mentioned, what bridges over the gulf of 1500 years between the composition of the book and the earliest events therein recorded? If books, who preserved them, and who guarantees their authenticity or their very existence? If tradition, in how far can it be accepted? Suppose, for instance, that the only existing accounts of Julius Cæsar were found in a work ostensibly written A.D. 1500, would they not be looked upon with great and deserved suspicion?

The alleged Frisian chronology takes its rise from the submersion of the old land, Aldland or Atland, which extended "far to the west of Jutland, and of which Heligoland and the islands of North Friesland are the last barren remnants." This event, we are told, "is known by geologists as the Cimbrian flood." Now, that a great submergence of land may at one time or other have taken place where now the North Sea extends is not to be denied. It is supposed, with good show of reason, that Britain was once part and parcel of the European continent, but has been severed either by an extensive subsidence or by an alteration in the position of the earth's centre of gravity which deepened the waters in northern latitudes. But Dr. Ottema, on the faith of the "Oera Linda Book," seeks to identify this vanished Atland with Plato's Atlantis. It is true all earlier tradition

has placed the site of this region in the Atlantic to the West of Africa, and a modern geologist has even—on very strong grounds—suggested that it formed the lowlands of a continent of which the West Indies are the more mountainous parts now alone remaining above the sea-level. Nothing, however, will satisfy Dr. Ottema but that Atlantis was a north-western prolongation of Holland. To one important point he makes no reference. Greek tradition represents the inhabitants of Atlantis as organising a vast expedition for the subjugation of the countries bordering on the Mediterranean, but as being repulsed by the Athenians. In the "Oera Linda Book" the Frieslanders are always represented as coming on friendly errands. The story of the disappearance of Aldland, Atland, or, if the reader will, Atlantis, is highly interesting, and may be made to serve as a crucial test for the authenticity of the entire book. We shall therefore quote in full:—

"Before the bad time came our country was the most beautiful in the world. The sun rose higher, and there was seldom frost. The trees and shrubs produced various fruits which are now lost. In the fields we had not only barley, oats, and rye, but wheat which shone like gold, and which could be baked in the sun's rays. The years were not counted, for one was as happy as another.

"On one side we were bounded by Wr-alda's Sea, on which no one but us might or could sail; on the other side we were hedged in by the broad Twiskland (Germany), through which the Finda people dared not come, on account of the thick forests and the wild beasts.

"Eastwards our boundary went to the extremity of the East Sea,* and westward to the Mediterranean Sea; so that, besides the small rivers, we had twelve large rivers given us by Wr-alda, to keep our land moist and show our seafaring men the way to the sea. The banks of these rivers were at one time entirely inhabited by our people, as well as the banks of the Rhine from one end to another. Opposite Denmark and Jutland we had colonies, and a Burgtmaagd (probably in Norway and Sweden). Thence we obtained copper and iron, as well as tar and pitch, and some other necessaries. Opposite to us we had Britain, formerly Westland, with her tin-mines.

"Britain was the land of the exiles, who, with the help of their Burgtmaagd, had gone away to save their lives; but in order that they might not come back they were

* Doubtless the Baltic, still called Ost-see—East Sea—by the Germans.

tattooed with a B on the forehead, the banished with a red dye, and the other criminals with a blue. Moreover, our sailors and merchants had many factories among the Krekalanders and in Lybia. As our country was so great and extensive we had many different names.

"Those who were settled in the higher marches bounded by Twisklanden (Germany) were called Saxmannen, because they were always armed against the wild beasts and the savage Britons."

How the bad time came:—

"During the whole summer the sun had been hid behind the clouds, as if unwilling to look upon the earth. There was perpetual calm, and the damp mist hung like a wet sail over the houses and marshes. The air was heavy and oppressive, and in men's hearts was neither joy nor cheerfulness. In the midst of this stillness the earth began to tremble as if she was dying. The mountains opened to vomit forth fire and flames. Some sank into the bosom of the earth, and in other places mountains rose out of the plains. Aldland, called by the seafaring men Atland, disappeared, and the wild waves rose so high over hill and dale that everything was buried in the sea. Many people were swallowed by the earth, and others who had escaped the fire perished in the water.

"It was not only in Finda's land that the earth vomited fire, but also in Twiskland. Whole forests were burned one after the other, and when the wind blew from that quarter our land was covered with ashes. Rivers changed their course, and at their mouth new islands were formed of sand and drift.

"During three years this continued, but at length it ceased, and forests became visible. Many countries were submerged, and in other places land rose above the sea, and the wood was destroyed through the half of Twiskland. Troops of Finda's people came and settled in the vacant places. Our dispersed people were exterminated or made slaves."

The question now arises whether this catastrophe, which must more or less have been felt throughout Europe, can be traced and identified? The phenomena described are of a twofold nature: there is, on the one hand, a permanent deterioration of climate, the sun no longer attaining its former altitude, frosts—formerly rare—becoming common, and wheat ceasing to ripen. The first-mentioned change, if it took place at all, would in all probability have more or less affected the whole world, and would doubtless have been

recorded in the traditions of other nations. But how could such a change be effected? An alteration in the plane of the ecliptic could produce no such result if we take the entire year into consideration, since if the sun attained a less altitude at the one solstice it would be greater at the other. Nor could a change in the eccentricity of the earth's orbit. The only occurrence that could cause the sun to attain a lower general altitude at any place than had been formerly the case would be the removal of such place farther from the Equator, and consequently nearer to the Pole. Now, such changes have indeed been known to occur on a small scale in landslips and earthquakes, but that an entire country should execute a horizontal movement of translation is a supposition not warranted by recorded facts. That there may have been a time—or rather times—when frost was of rare occurrence in north-western Europe is highly probable. When France, Britain, and Germany displayed a semi-tropical flora, and when even Iceland, Lapland, and Spitzbergen had their forests of beeches and oaks, frost would doubtless be of rare occurrence; but there is no evidence that a period of this kind fell so recently as 2190 B.C., or, in other words, 4000 years ago. Here, then, is a decidedly mythological feature—the transposition of an event from its own to a later date.

The account of the "bad time" is, further, inconsistent, for though frosts now occur over most of the countries claimed as parts of the old Frisian dominion, yet wheat still ripens in every one of the countries specified, with the exception, perhaps, of Scandinavia. As to a heat sufficient to bake the wheat in the fields, we need scarcely say that since the earth became habitable no such temperature ever existed. On the other hand the "bad time" consisted in an alteration of the surface of the earth; portions of land were submerged, such as Atland, and in their stead land arose where formerly sea had existed. The case, therefore, was neither subsidence nor elevation, but a combination of both, and could not have been produced by a change in the position of the earth's centre of gravity, and a consequent transference of the main volume of the waters from one hemisphere to the other. Had it fallen two centuries earlier it might possibly have been identified by divines with the Noachian deluge.

The geography of the story is also somewhat obscure. If the Frisians occupied the country "to the extremity of the East Sea" and the banks of the Rhine from one end to the other, where was Twisklanden, the country of the

Germans, with its dense forests? It is also remarkable that the Frisians settled in the higher marches bordering on Germany—consequently to the east of Friesland—"were always" armed against the savage Britons (Vrwilderda Britne). How could the Britons attack Friesland from this side?

The following passage seems to prove that Dr. Ottema has not studied very carefully the book for which he stands sponsor:—"With regard to mythology this writing, which bears no mythical character, is not less remarkable than with regard to history, Notwithstanding the frequent and various relations with Denmark, Sweden, and Norway, we do not find any traces of acquaintance with the Northern or Scandinavian mythology. Only Wodin appears in the person of Wodan, a chief of the Frisians, who became the son-in-law of one Magy, king of the Finns, and after his death was deified." Yet on the very next page we read that Frya, the grand-daughter of the eternal and almighty Wr-alda* (query, *Ur-alte*, the primeval one), is the mother of Frya's people, the Frisians, and is reverenced as the representative of Wr-alda. Can Dr. Ottema have overlooked that this is none other than the Scandinavian Venus, Freya, or Friga, to whom the sixth day of the week was consecrated by our forefathers? Again, we read in the "Oera Linda Book" of Irtha, the earth, daughter of Wr-alda, and mother of Frya. Is not this the Hertha who was worshipped by the Germans, and whose consecrated chariot was preserved in the Isle of Rügen down to the times of Charlemagne? Then we have mention of Walhallagera as a city of importance. Does not this remind the reader of Valhalla, the gara or gardt (citadel) of the gods?

But whilst Dr. Ottema thus ignores the very palpable connection between his book and the Teutonic mythology, he finds in it the "closest connection" with Greek and Roman legends, and even derives from it an account of the "origin of two deities of the highest rank, Min-erva and Neptune. Minerva (Athene) was originally a Burgtmaagd, priestess of Frya, at the town of Walhallagara, Middelburg, or Domburg, in Walcheren. The other, Neptune, called by the Etrurians Nethunus, the god of the Mediterranean Sea, appears to have been, when living, a Friesland Viking, or sea-king, whose home was Alderga (Ouddorp, not far from Alkmaar). His name was Teunis, called familiarly by

* Named also, in one passage, Alvader, *i.e.*, All-father, the true name of the Supreme among the Teutonic nations.

his followers Neef Teunis, or Cousin Teunis, who had chosen the Mediterranean as the destination of his expeditions, and must have been deified by the Tyrians at the time when the Phenician navigators begun to extend their voyages so remarkably, sailing to Friesland (!) to obtain British tin, northern iron, and amber from the Baltic, about 2000 years before Christ.

"Besides these two, we meet with a third mythological personage—Minos, the law-giver of Crete, who likewise appears to have been a Friesland sea-king; Minno, born at Lindacord, between Wiesingen and Kreyl, who imparted to the Cretans an 'Asagaboek.' He is that Minos who, with his brother Rhadamanthus and Aeacus, presided as judge over the fates of the ghosts in Hades, and must not be confounded with the later Minos, the contemporary of Ægeus and Theseus, who appears in the Athenian fables.

"The reader may perhaps be inclined to laugh at these statements, and apply to me the words that I myself have lately used, fantastic and improbable. Indeed at first I could not believe my own eyes, and yet, after further consideration, I arrived at the discovery of extraordinary conformities, which render the case much less improbable than the birth of Min-erva from the head of Jupiter, by a blow from the axe of Hephaestus, for instance."

If the reader has managed so far to restrain his laughter we fear that his gravity will be completely upset by the exquisite simplicity of the sentence last quoted. Can Dr. Ottema really think that any one in these days conceives of Minerva as a veritable personage born from the brain of Jupiter? Does he so ill comprehend the nature of the myth as to think his own story must find acceptance because historically "less improbable"? If Minerva was a Frisian priestess who settles in Attica and builds Athens, how comes it that her name should survive in Italy and be replaced in Greece by Pallas? How, if Pallas came from Friesland, is she likely to have endowed Attica with the olive? "Cousin Teunis," as the etymology of Neptune, strongly reminds us of Dean Swift's derivation of the name Achilles, from "a kill ease." Indeed we are not without suspicions that the entire "Oera Linda Book" may be a solemn Dutch joke, and that the learned Doctor is gravely chuckling at his believing readers. We are somewhat startled to read of the Phenicians sailing to Friesland in order to obtain British tin. We know, from the most positive facts, that they came direct to Cornwall, where coins, local names, and evident traces of Phenician blood—as pointed out by Dr. Knox and

other ethnologists—attest their former presence. For them to sail past Britain to Friesland for a British commodity would be as absurd as for a British ship in quest of tea to sail past China to Jesso.

One name occurring in the book might, we think, have been utilised by Dr. Ottema. The Priestess Adela says—"I refused to be Volcksmœder because I wished to marry Apol." We think that, with a little trouble and a few bold conjectures, this Apol might be identified with Apollo, even though the "God of life, and poetry, and light" is generally represented as having been a bachelor.

Neptune—we beg pardon, Cousin Teunis—had, it seems, a cousin and fellow-adventurer, one Inka. On one of their expeditions to the Mediterranean, after touching at Kadik (Cadiz), then a Frisian colony, they disagreed. "Teunis wished to sail through the Straits to the Mediterranean Sea, and enter the service of the rich Egyptian king, as he had done before, but Inka said that he had had enough of all those Finda's people. Inka thought that perhaps some high-lying part of Atland might remain as an island, where he and his people might live in peace. As the two cousins could not agree, Teunis planted a red flag on the shore and Inka a blue flag. Every man could choose which he pleased, and to their astonishment the greater part of the Finns and Magyars followed Inka, who had objected to serve the kings of Finda's people. When they had counted the people, and divided the ships accordingly, the fleet separated. We shall hear of Teunis afterwards, but nothing more of Inka."

We fear that Dr. Ottema has here, again, lost an opportunity. Is it not self-evident—to the enthusiastic at least—that Inka must have sailed away to the west, and discovered South America, where he founded the Peruvian Empire, which his descendants long ruled under the name of the Incas? Is it not, however, strange that so maritime a people as the Frisians, 180 years after the submergence of Atland, are represented as still in doubt whether any part of that region had remained as an island?

We further learn that Marseilles and Tyre were originally Frisian colonies, as well as Cadiz and Athens. From the latter they were ultimately expelled by Cecrops, a Frisio-Egyptian half-breed, represented as "bright of eye, clear of brain, and enlightened of mind." This leads us to one of the strangest passages in the entire work:—"Geert, the Frisian priestess of Athens, departed with the best of Frya's sons and seven times twelve ships. Soon after they had left the harbour they fell in with at least thirty ships coming

from Tyre, with women and children. They were on their way to Athens, but when they heard how things stood there they went on with Geert. The sea-king of the Tyrians brought them through the Strait, which at that time ran into the Red Sea (now re-established as the Suez Canal). At last they landed at the Punjab, called in our language the Five Rivers, because five rivers flow together into the sea. Here they settled, and called it Geertmania. The King of Tyre afterwards, seeing that all his best sailors were gone, sent all his ships with his wild soldiers to catch them, dead or alive. When they arrived at the Strait both the sea and the earth trembled. The land was upheaved, so that all the water ran out of the Strait, and the muddy shores were raised up like a rampart. This happened on account of the virtues of the Geertmen, as every one can plainly understand." Thus it would appear that at one time the Red Sea communicated with the Mediterranean, and that Africa was an island, but that in B.C. 1551, or about that time, the passage was closed up. If the exodus of the Israelites out of Egypt is taken at the date ordinarily assigned to it, viz., B.C. 1564, we shall see that this region within some thirteen years witnessed the flight of two nations, each saved by a miracle of a converse nature. The Israelites, fleeing on land, are preserved by the influx of waters; the Frisians, escaping by sea, are saved by a rampart of land upheaved behind them! This is, to say the least, a very curious coincidence. Dr. Ottema very zealously supports this view of the Frisian colonisation of India. He says in his Introduction:—

"The historians of Alexander's expedition do not speak of Frisians or Geertmen, though they mention Indoscythians, thereby describing a people who live in India, but whose origin is in the distant unknown North.

"In the accounts of Lindgert no names are given of the places where the Frisians lived in India. We only know that they first established themselves to the east of the Punjab, and afterwards moved to the west of these rivers. We find in Ptolemy, exactly 24° N. on the west side of the Indus, the name Minnagara; and about 6 degrees east of that, in 22° N., another Minnagara. This name is pure Fries, the same as Walhallagara, Folsgara, and comes from Minna, the name of an Eeremœder (high-priestess), in whose time the voyages of Teunis and his nephew Inca took place.

"This coincidence (!) is too remarkable to be accidental, and not to prove that Minnagara was the head-quarters of

the Frisian colony. The establishment of the colonists in the Punjab in 1551 before Christ, and their journey thither, we find fully described in Adela's book, and with the mention of one most remarkable circumstance, namely, that the Frisian mariners sailed through the Strait which in those times still ran into the Red Sea."

"In Strabo (Book I., pp. 38 and 50) it appears that Eratosthenes was acquainted with the existence of the Strait, of which the later geographers make no mention. It existed still in the time of Moses (Exodus xiv., 2), for he encamped at Pi-ha-chiroht, the 'Mouth of the Strait.' Moreover, Strabo mentions that Sesostris made an attempt to cut through the isthmus, but that he was not able to accomplish it. That in very remote times the sea really did flow through it is proved by the result of the geological investigations on the isthmus made by the Suez Canal Commission, of which M. Renaud presented a Report to the Academy of Sciences, on the 19th June, 1856. In that Report, among other things, appears the following:—'Une question fort controversée est celle de savoir si à l'époque où les Hebreux fugaient del 'Egypte sous la conduite de Moïse les lacs amers faisaient encore partie de la Mer Rouge. Cette dernière hypothêse s'accorderait mieux que l'hypothèse contraire avec le texte des livres sacrés, mais alors il faudrait admettre que depuis l'époque de Moïse le seuil de Suez serait sorti des eaux.'"

This entire passage is remarkable for its loose method of reasoning. That at some remote period there may have been a connection between the Red Sea and the Mediterranean is perfectly possible; but the question here at issue is, did such Strait exist down to the time of Moses, and disappear thirteen years after? The expression "Mouth of the Strait" proves nothing, since it may signify a narrow sea closed at one end, like either of the two northern forks of the Red Sea. We think that everyone who reads the Book of Genesis without prepossession must conclude that in the time of the early Hebrew patriarchs the road from Palestine to Egypt did not involve the passage of an arm of the sea. The attempt of Sesostris, as mentioned by Strabo, proves nothing, since such a canal would be equally useful whether the isthmus had existed from time immemorial or was a mere recent upheaval. Nor can much weight be laid on the name of Minnagara. That such a name, as given through the medium of a Greek author, and probably modified, may be found capable of a Frisian etymology—involving, too, a proper name—is a coincidence too trifling to be of any

weight. We cannot lay claim to any philological authority, but those most profoundly versed in the science of language declare that the languages of India and of Western Europe all belong to one great family, and point to one common root. Where, then, is the wonder if an Indian name is capable of receiving a Frisian etymology?

In the account of the visit of Ulysses to Holland there is one very peculiar circumstance. The Frisians throughout this book are represented pre-eminently as the seafaring people of the world—the only nation who could or might navigate the sea of Wr-alda, or the great *ocean*. Yet the ships of Ulysses are described as "finer than any that we possessed or had ever seen." This, too, after having been driven about for years in strange seas, where they must have been damaged and shattered, and where they could have found but few opportunities for refitting.

Among the further suspicious features of the "Oera Linda Book" we are struck with its manner of appealing to that national pride in which, despite his phlegma, Mynheer is no wise deficient.* To the foreigner, Holland seems generally—perhaps too generally—a mere swampy appendage of Germany which has accidentally acquired independence, and its language a mere vulgarised form of German, or, as it has been humorously described, "German spoken during a bad fit of sea-sickness." Even such an author as Jean Paul Richter could so far forget the honourable and splendid part taken by Holland during the sixteenth, seventeenth, and eighteenth centuries, in the development of science, as to characterise the Dutchman as a "cheap edition of the German, printed on inferior paper, and without illustrations." But to the Dutchman who receives Dr. Ottema's revelations his country is no mere fragment of Germany, but the relic of a vast region to which imagination supplies the only limit—the originator of alphabetical writing and of our arithmetical notation, the instructress and civiliser of Greece, of Hetruria, perhaps of Phenicia, great alike in arms, in legislation, and in commerce, and planting her institutions even as far as the Punjab! We must further notice the side-blows dealt at the three mighty neighbours of Holland. France has proved herself dangerous and aggressive alike under Louis le Grand and under the first Napoleon. Ac-

* Witness the indignation excited by Washington Irving's "History of New Amsterdam." Within the lifetime of the present generation a mere casual reference to this elegant writer has been known, in Dutch Society, to bring on the same ominous silence as might be provoked in an English drawing-room by the introduction of some indecent topic.

cordingly we find mention of the "dirty Gauls." Germany is naturally a cause of dread and suspicion. Hence the "Twisker" are stigmatised in advance as "robbers." England has outstripped Holland in navigation, commerce, and colonisation. Therefore the "Oera Linda Book" dolefully relates how the Frisians lost Britain, once their penal colony, and how their rule anticipated ours in the Punjab. Can we imagine anything more soothing to Dutch patriotism?

Another remarkable and suspicious circumstance is the hostility to priests and priestcraft pervading the whole document, and appearing even in a note appended by a former possessor of the book. Liko, surnamed Over de Linda, in the year A.D. 803, is made to write as follows:—"Beloved successors, for the sake of our dear forefathers and of our dear liberty I entreat you, a thousand times, never let the eye of a monk look on these writings." Now, that monks have destroyed many documents which would have thrown a priceless light on history and ethnology needs not to be contested. Nor is it, perhaps, altogether incredible that this fact should have been recognised as early as the year A.D. 803. But we can scarcely imagine a general and systematic dread of priestcraft before ecclesiastical power was formally organised, and before its "alethophobia"—as a modern French writer terms it—had been developed. It is, moreover, strange that a nation whose government might be called a theocracy directed by priestesses should show such a repugnance to priests. This feature, we think, points not doubtfully to a very modern origin of the work.

Dr. Ottema insists that "there is a striking difference between this book and the Greek myths. The Myths have no dates, much less any chronology, nor any internal coherence of successive events. The untrammelled fancy develops itself in every poem separately and independently. The mythological stories contradict each other on every point. 'Les Mythes ne se tiennent pas' is the only key to the Greek mythology.

"Here, on the contrary, we meet with a regular succession of dates starting from a fixed period—the destruction of Atland, 2193 before Christ. The accounts are natural and simple, often *naïve*, never contradict each other, and are always consistent with each other in time and place. As, for instance, the arrival and sojourn of Ulysses with the Burgtmaagd Kalip at Walhallagara (Walcheren), which is the most mythical portion of all, is here said to be 1005 years after the disappearance of Atland, which coin-

cides with 1188 years before Christ, and thus agrees very nearly with the time at which the Greeks say the Trojan war took place.

"Another remarkable difference consists in this, that the Myths know no origin, do not name either writers or relators of their stories, and therefore never can bring forward any authority; whereas in Adela's book for every statement is given a notice where it was found or whence it was taken. For instance—'This comes from Minno's writings—this is written on the walls of Waraburch—this is in the town of Frya—this at Stavia—this at Walhallagara.'

"There is also this, further. Laws, regular legislative enactments, such as are found in great numbers in Adela's book, are utterly unknown in Mythology, and indeed are irreconcilable with its existence. Even when the Myth attributes to Minos the introduction of law-giving into Crete, it does not give the least account of what the legislation consisted in. Also, among the gods of mythology there existed no system of laws. The only law was unchangeable Destiny and the will of the supreme Zeus."

Now, as regards the alleged non-mythical character of the "Oera Linda Book," we point out elsewhere some instances to the contrary which seem to have escaped Dr. Ottema's notice. But the entire cosmogony contained in the "Book of Adela's Followers" is mythical in the fullest sense of the word. Hertha gives birth to three maidens :—

"Lyda out of fierce heat.
"Finda out of strong heat.
"Frya out of moderate heat.

"When the last came into existence Wr-alda breathed his spirit upon her, in order that men might be bound to him. As soon as they were full-grown they took pleasure and delight in the visions of Wr-alda.

"Hatred found its way among them. They each bore twelve sons and twelve daughters—at each Jule-time a couple. Thence come all mankind."

This surely is of the very essence of mythology, as is the next following account of the apotheosis of Frya and her transfer to a watch-star. But admitting that the "Oera Linda Book" differs in some points from the majority of mythological legends, we must draw from this circumstance a conclusion widely differing from that of Dr. Ottema. The references to authorities and the exact dates are, in our opinion, no mean testimony in favour of its being a supposititious production.

Meantime we cannot admit that entire freedom from contradictions for which Dr. Ottema contends. On p. 33 we read :—" In early times almost all the Finns (in the original Finda's folk) lived together in their native land, which was called Aldland, and is now submerged. They were thus far away, and we had no wars. When they were driven hitherwards, and appeared as robbers, then arose the necessity of defending ourselves, and we had armies, kings, and wars."

Yet, judging from other portions, Aldland or Atland was the patrimony of the Frisians, and its submergence " occasioned a great dispersion of the Frisian race." Moreover, if it was land stretching out to the west of Jutland, and if Heligoland and the isles of North Friesland, are its " last barren remains," it could in no manner be pronounced " far away," having been part and parcel of what is now known as Holland. Indeed the account of the origin of the Finns or Finda's folk, as given in detail on p. 73, fully contradicts the view of their having been the original inhabitants of Aldland :—

" One hundred and one years *after the submersion of Aldland* a people came *out of the East.* That people was driven by another. Behind us, in Twiskland (Germany), they fell into disputes, divided into two parties, and each went its own way. Of the one no account has come to us, but the other came in the back of our Schoonland (Skenland, or Scandinavia), which was thinly inhabited, particularly the upper part. Therefore they were able to take possession of it without contest, and as they did no other harm we would not make war upon them. They were not wild people like most of Finda's race, but, like the Egyptians, they have priests and also statues in their churches. The priests are the only rulers, and call themselves Magyars and their head man Magy : he is high priest and king in one. The rest of the people are of no account, and in subjection to them. This people have not even a name, but we call them Finns (Finna) because, though all the festivals are mournful and bloody, they are so formal that we are inferior to them in that respect." This passage decidedly proves the contradiction to which we have just referred.

As regards the coincidences upon which Dr. Ottema lays such emphasis, they are striking only if we start with the assumption that the book is a genuine document. If we consider it a forgery, they as decidedly support that hypothesis. Besides, it must be noted that the work gives us, after all, merely what may be called modified versions of old stories. There is nothing entirely new; nothing which

might not have been invented by an accomplished literary forger of the present day. We might naturally have expected that a people so civilised as the early Frisians are represented, even 2000 years before the Christian era, would have had something definite to communicate concerning their own origin and early history. But except the mythical account of their descent from Frya, and the laws received from her by revelation, we are left greatly in the dark. Whence came the Frisians? Were they truly aborigines, created or evolved upon the spot? We feel the more disposed to ask this question because, if the "Oera Linda Book" is to be trusted, our present views on the history of civilisation and on ethnology must be reconsidered. This work decidedly combats the prevailing view that Europe received its civilisation from Asia, culture being gradually extended westwards. On the contrary, it represents Eastern Europe and Western Asia as receiving arts, letters, and laws from the North-west. Surely before views thus contrary to well-known facts can be ascertained, the strictest scrutiny must be demanded.

There is a further consideration drawn, not from the contents of the book, but from its alleged history. Had the manuscript been lying, overlooked and forgotten, in the library of some convent or university, or in the muniment-room of some old or rarely-visited castle, its resurrection in the latter part of the nineteenth century would not have been, *prima facie*, improbable; but it is described as in the custody of a private family who knew of its existence and ascribed to it no little importance. It is specially bequeathed to the present holder by his grandfather. And all this in a highly educated country which has been for centuries honourably distinguished for its intellectual activity, and where men of learning abound! Can we imagine the possessors of this mysterious document feeling no curiosity as to its contents? Would they not naturally have consulted some archæologist or philologian, and would not the world long ago have been in possession of the secret?

That the book is interesting, and that the sentiments expressed are of a highly moral—we might almost say of a suspiciously moral—tendency, there is no occasion to dispute.

What may be the true origin of the "Oera Linda Book" we do not profess to decide. It may be a romance, a hoax, or an intentional and downright imposition. Or it may be a *saga* adroitly interpolated and manipulated. But we certainly cannot accept it as a genuine and authentic historical record.

Concerning the translation little need be said. Mr. Sandbach thinks that if the book is allowed to be 100 or 150 years old, there is no reason for refusing it a greater antiquity. He thinks there is "nothing in the narratives of this book inconsistent with probability," and he endorses the absurd remark of Dr. Ottema that it is not more improbable for a "clever woman to have become a lawgiver at Athens than for a goddess to spring, full-grown and armed, from the cleft skull of Jupiter!" *Sancta simplicitas!*

III. THE CONSTANTS OF COLOUR.

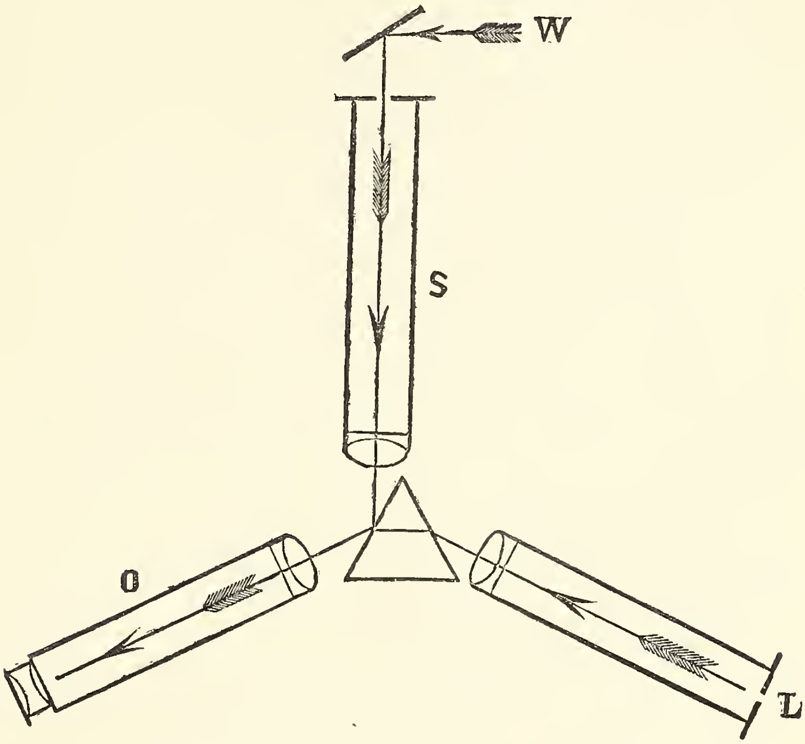
By Professor O. N. Rood, of Columbia College.

THE tints produced by Nature and Art are so manifold, often so vague and indefinite, so affected by their environment, or by the illumination under which they are seen, that at first it might well appear as though nothing about them were constant; as though they had no fixed properties which could be used in reducing them to order, and in arranging in a simple but vast series the immense multitude of which they consist.

Let us examine the matter more closely. We have seen that when a single set of waves acts on the eye a colour-sensation is produced, which is perfectly well defined, and which can be indicated with precision by referring it to some portion of the spectrum. We have also found that when waves of light having all possible lengths act on the eye simultaneously, the sensation of white is produced. Let us suppose that by the first method a definite colour-sensation is generated, and afterward by the second method the sensation of white is added to it; white light is added to or mixed with coloured light. This mixture may be accomplished with an ordinary spectroscope, by removing the scale from the scale-telescope, and replacing it by a vertical slit, as indicated in Fig. 1, which is a view from above. Then if white light be allowed to enter this slit, it will be reflected from the surface of the prism into the observing-telescope, and we shall find that the spectrum is crossed by a vertical band of white light. By moving with the hand the scale-telescope, this white band may be made to travel slowly over the whole spectrum, and furnish us with a series of mixtures of white light with the various

prismatic tints. (See Fig. 2.) The general effect of this proceeding will be to diminish the action of the coloured light; the resultant light will indeed present to the eye

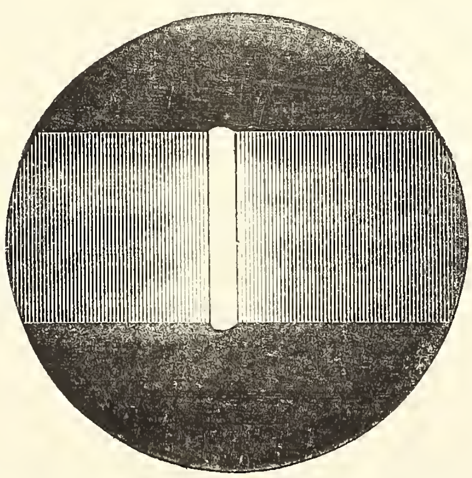
Fig. 1.



O is the observing-telescope; S, the scale-telescope; L, the source of light which furnishes the spectrum; W, the white light which is projected on the spectrum.

more light, but it will appear paler; the colour-element will begin to be pushed into the background. Conversely, if we now should subject our mixture of white and coloured light

Fig. 2.



Spectrum crossed by Band of Superimposed White Light.

to analysis by a second spectroscope, we should infallibly detect the presence of the white as well as of the coloured light;

or, if no white light were present, that would also be equally apparent.

Taking all this into consideration, it is evident that when a particular colour is presented to us we can affirm that it is perfectly pure, viz., entirely free from white light; or that it contains mingled with it a larger or smaller proportion of this foreign element. This furnishes us with our first clue toward a classification of colours: our pure standard colours are to be those found in the spectrum; the coloured light coming from the surfaces of natural objects, or from painted surfaces, we must compare with the tints of the spectrum. If this is done, in almost every case the presence of more or less white light will be detected; in the great majority of instances its preponderance over the coloured light will be found quite marked. To illustrate by an example:—If white paper be painted with vermillion, and compared with a solar spectrum, it will be found that it corresponds in general tone with a certain portion of the red space; but the two colours never match perfectly, that from the paper always appearing too pale. If, now, white light be added to the pure spectral tint, by reflecting a small amount of it into the observing-telescope, it will become possible to match the two colours, and, if we know what proportion of white light has been added, we can afterward say that the light reflected from the vermillion consists, for example, of 80 per cent of red light from such a region of the spectrum, *plus* 20 per cent of white light. If we set the amount of light reflected by white paper as 100, then a surface painted with "emerald-green" reflects about 8 parts of white light; artificial ultramarine, 2 or 3 parts; red-lead, 7 or 8, &c. Some white light is always present: its general effect is to soften the colour and reduce its action on the eye: when the proportion of white is very large, only a faint reminiscence of the original hue remains; we say the tint is greenish grey, bluish grey, or reddish grey. The specific effects produced by the mixture of white with coloured light will be considered hereafter; it is enough for us at present to have obtained an idea of one of the constants of colour, viz., its purity. The same word, it may be observed, is often used by artists in an entirely different sense: they will remark of a painting that it is noticeable for the purity of its colour—meaning only that the tints in it have no tendency to look dull or dirty, but not at all implying the absence of white or grey light.

Next let us suppose that in our study of these matters we have presented to us for examination two coloured

surfaces, which we find reflect in both cases eight-tenths red light and two-tenths white light. In spite of this the tints may not match, one of them being much brighter than the other, containing, say, twice as much red light and twice as much white light; having, in other words, twice as great brightness or luminosity. The only mode of causing the tints to match will be to expose the darker-coloured surface to a stronger light, or the brighter surface to one that is feebler. It is evident then, that brightness or luminosity is one of the properties by which we can define colour; it is our second colour constant. This word luminosity is also often used by artists in an entirely different sense, they calling colour in a painting luminous, simply because it recalls to the mind the impression of light, not because it actually reflects much light to the eye. The term "bright colour" is sometimes used in a somewhat analogous sense, but the ideas are so totally different that there is little risk of confusion.

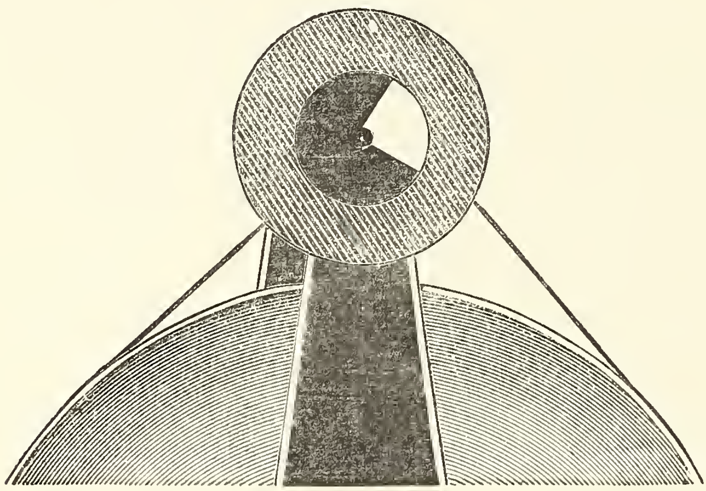
The practical determination of the second constant is possible in a great many cases; it presents itself always in the shape of a rather troublesome photometric problem, capable of a more or less accurate solution. The relative brightness of the colours of the solar spectrum is one of the most interesting of these problems, as its solution would serve to give some idea of the relative brightness of the colours which, taken together, constitute white light. Quite recently a set of measurements were made in different regions of the spectrum by Vierordt, who denoted the points measured by the fixed lines, as is usual in such studies.* The following table will serve to give an idea of his results:—

Colour.	Degree of Luminosity.
Dark red	800
Red	4,930
Red, slightly orange	11,000
Orange-red	27,730
Orange	69,850
Yellow	78,910
Green	30,330
Cyan blue	11,000
Blue	4,930
Ultramarine blue	906
Violet	359
,,	131
,,	58
,,	9

* C. Vierordt, Poggendorff's Annalen, Band cxxxvii., S. 200.

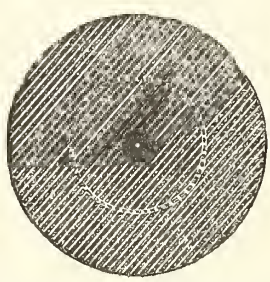
These measurements were made on a spectrum obtained by a glass prism, which, as has been mentioned previously, contracts the red, orange, and yellow spaces unduly, and hence increases their illumination disproportionately. It is to be hoped that a corresponding set of measurements will soon be made on the normal spectrum, furnished by a ruled plate. If we should multiply the luminosity of the colours in either kind of spectrum by their extent or areas, we should obtain measures of the relative amounts of these several tints in white light.

Fig. 3.



Coloured Disc with Small Black-and-White Disc.

Fig. 4.

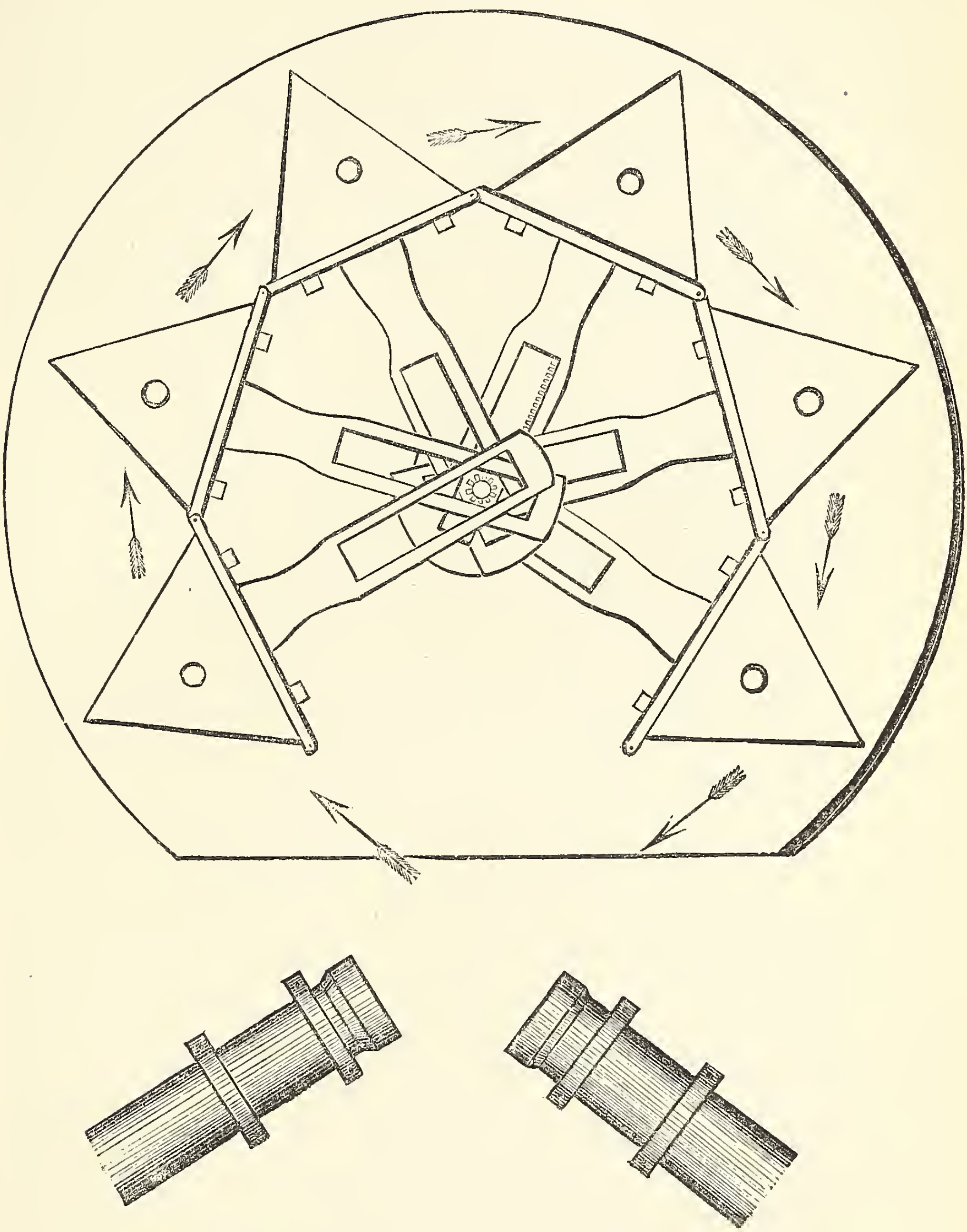


Coloured Disc with Small Black-and-White Disc in Rotation.

By the simple method of rotating discs we can very roughly determine the second constant in the case of a coloured surface, for example, of paper tinted with vermillion. A circular disc, about 6 inches in diameter, is cut from the paper, and placed on a rotation apparatus, as indicated in Fig. 3. On the same axis is fastened a double disc of black-and-white paper, so arranged that the proportions of black and white can be varied at will. When the whole is set in rapid rotation, the colour of the vermillion paper will of course not be altered, but the black and white will

blend into a grey. This grey can be altered in its brightness, till it *seems* about as luminous as the red. If we find, for example, that with the disc three-quarters black and one-quarter white, an equality appears to be established, we conclude that the luminosity of our red surface is 25 per cent

FIG. 5.



Facsimile of Rutherford's Drawing of Six-Prism Spectroscope.—(*American Journal of Science and Arts*, 1865.)

of that of white paper. This of course based on the supposition that the black paper reflects no light; it actually reflects from 2 to 5 per cent, the reflecting power of white paper being put at 100. The results thus obtained are always inexact, and the same observer will often obtain

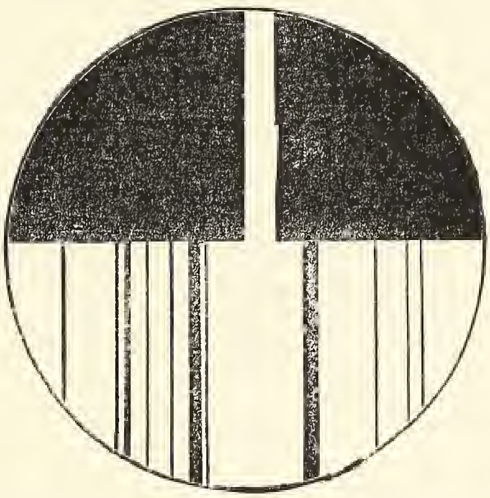
different results on different days, though those of a single day may agree pretty well among themselves. In the appendix a peculiar photometer will be described, which has been contrived by the author for the purpose of comparing more accurately together the relative luminosity of different coloured surfaces, or that of coloured and white surfaces.

But to resume our search for colour-constants. We may meet with two portions of coloured light, having the same degree of purity and the same apparent brightness, which nevertheless appear to the eye totally different; one may excite the sensation of blue, the other that of red; we say the *tones* are entirely different. The *tone* of the colour is then our third and last constant, or, as the physicist would say, the degree of refrangibility, or the wave-length of the light. It has previously been shown that the spectrum offers all possible tones except the purples, well arranged in an orderly series; and the purples themselves can be produced with some trouble, by causing the blue or violet of the spectrum to mingle in certain proportions with the red. Rutherford's automatic six-prism spectroscope can very conveniently be employed for the determination of the tone. (See Fig. 5.) A peculiar eye-piece is to be used, which isolates a little slice of the spectrum in its upper half, as indicated in Fig. 6. In the lower half of the field the fixed lines are seen, and the tone selected as matching the colour under examination can be located by their aid. Afterward, if it is considered desirable, white light can be added to the spectral tint, till it is subdued sufficiently to render exact comparison possible.

The experimental determination of the colour-constants is beset with a considerable amount of difficulty, even in the simplest cases, such as cardboards covered with pigments. The best mode of proceeding appears to be to call the luminosity of white cardboard 100, and then to determine photometrically the comparative luminosity of the coloured cardboards. The measurement of the amount of white light reflected along with the coloured is still more troublesome, and the result likely to be somewhat less exact, while the determination of the tone, or third constant, is moderately easy under favourable circumstances. One of the uses of such determinations is, the production of a set of standard coloured discs with known constants, which can afterward be combined with each other, as well as with standard black or white discs, so as to generate at will, with ease and certainty, an immense number of tints whose constants will be

known. If we make a record of the constants involved in such experiments we can afterward reproduce the tints just as they originally were, or alter them to any desirable extent. To carry out the letter of this it will of course be necessary to view the standard discs under similar illuminations at different times—a point which can be secured with the aid of the photometer above referred to. The standard discs can also be used for building up a set of standard charts, containing a vast variety of tints of known composition, arranged methodically with regard to purity, luminosity, and tone. These matters will be considered at some length in a separate chapter, and are now only hinted at as a justification for the trouble we have been at in defining the constants of colour.

FIG. 6.



Eyepiece for Isolating the Tints of the Spectrum.

There is another point to be touched on in this connection. One of the most noticeable things about colours is their difference in *intensity*. Colours are intense when they excel both in purity and brightness; for it is quite evident that, however pure the coloured light may be, it still will produce very little effect on the eye if its total quantity be small; and, on the other hand, it is plain that its action on the same organ will not be considerable if it is diluted with much white light. Purity and brightness, or luminosity, are, then, the factors on which intensity depends. We shall see hereafter that this is strictly true only within certain limits, and that an inordinate increase of luminosity is attended with a loss of intensity of hue.

Having defined the three constants of colour, it will be interesting to inquire into the sensitiveness of the eye in these directions. This subject has lately been studied with

care by Aubert, who made an extensive set of observations with the aid of coloured discs.* It was found that the addition of 1 part of white light to 360 parts of pure coloured light produced a change which was perceptible to the eye; smaller amounts failed to bring about this result. It was also ascertained that mingling pure coloured light with from 120 to 180 parts of white light caused it to become invisible, the hue being no longer distinguishable from white. Differences in luminosity as small as 1-120 to 1-180 could under favourable circumstances be perceived. It hence followed that irregularities in the illumination or distribution of pigment over a surface, which were smaller than 1-180 of the total amount of light reflected, could no longer be noticed by the eye. Experiments with red, orange, and blue discs were made on the sensitiveness of the eye to changes of tone or refrangibility: thus the combination of the blue disc with a minute portion of the red disc altered its hue by moving it a little toward violet; on reversing the case, or adding a little blue to the red disc, the tone of the latter moved in the direction of purple. Similar combinations were made with the other discs. Aubert ascertained, in this way, that recognisable changes of tone could be produced by the addition of quantities of coloured light as small as from 1-100 to 1-300 of the total amount of light involved. From such data he calculated that in a solar spectrum at least 1000 distinguishable tones are visible. But we can still recognise these tones when the light producing them is subjected to considerable variation in brightness. Let us limit ourselves to 1000 slight variations, which we can produce by gradually increasing the brightness of our spectrum, till it finally is ten times as luminous as it originally was. This will furnish us with a million tones, differing perceptibly from each other. If each of these tones is again varied 300 times, by the addition of different quantities of white light it carries up the number of hues we are able to distinguish as high as 300,000,000. In this calculation no account is taken of the purples, or of colours which are very bright or very faint, or mixed with very much white light. For these it will hardly be extravagant to demand another 100,000,000; we reach thus the astonishing conclusion that the human eye under favourable circumstances is able to distinguish as many as 400,000,000 different hues!—*From Advance Sheets of the New York "Popular Science Monthly."*

* AUBERT, Physiologie der Netzhaut. Breslau, 1865.

IV. THE ENCOURAGEMENT OF SCIENTIFIC RESEARCH.*

AN opinion is rapidly gaining ground that the present scientific position of Britain is unsatisfactory, both as compared with that of certain foreign nations and with our own antecedents, and is consistent neither with the honour nor with the true interests of the Empire. To escape misunderstanding or misrepresentation we must endeavour to state the case, as it appears to us, with more precision. We do not deny that modern England can boast a series of names which, if equalled elsewhere, are surpassed nowhere. We believe that in *scientific ideas* we are on a level with the foremost nations of the world; that in speculative philosophy we have re-conquered the foremost place, whilst in biology we have initiated a complete regeneration. We have the greater pleasure in pointing out these saving considerations since they prove that our shortcomings are due not to any natural deficiencies in the British and Irish mind, but to circumstances fortuitously or artificially created, and which may of course be artificially modified. What we complain of, then, relates not to the height of our *scientific ideas*, but to the quantity of our *scientific work* and the number of our earnest and qualified *scientific workers*. On this point there is scarcely room for discussion. Even the most pseudo-patriotic John Bull who accuses us of undervaluing our own country can scarcely deny the facts which we have to bring forward. Let us look at our scientific literature; it is exceedingly rich in the mere number of books published; but what an overwhelming proportion of them—as every reviewer knows to his sorrow—are mere compilations, elementary treatises, and the like, well-known matter brought forward again and again in a slightly modified form. How many of the original works, even, are original in little save absurdity, and consist in wild attempts to subvert the whole existing system of our knowledge and rebuild it as if by magic. Let us turn to the original papers announcing some discovery of greater or less moment, and appearing in the Transactions or Journals of learned societies, and in the various scientific periodicals. Here our comparative poverty is most striking. Let us take up, for instance, the "Berichte"

* Essays on the Endowment of Research. By Various Writers. London: H. S. King and Co.

of the German Chemical Society, or Liebig's "Annalen." We see there reports of investigations conducted in the Laboratories of the different Universities—Heidelberg, Bonn, Berlin, Göttingen, Jena, even Greifswalde—which in our college-days was ridiculed as a mere colony of louts dismissed from other seats of learning as intellectually or morally incapable. But when do we find in the "Journal of the Chemical Society" or in the "Chemical News" a series of valuable notes under the heading "Researches from the Laboratory of the University of Oxford," or of Cambridge, or of Durham, or of Dublin? And if not, why not? But leaving Germany out of the question, even the Russian Universities are now contributing their regular quota of research towards the world's sum of knowledge.

The following evidence, given by Dr. Frankland before the Royal Commission on Scientific Instruction, is painfully significant*:—"A year or two ago I took the trouble to look out, in regard to chemistry, the number of original investigations made in each country during one year. In the year 1866, which was the year I enquired, 1273 papers were published by 805 chemists. Of these Germany contributed 445 authors and 777 papers; France, 170 authors and 245 papers; the United Kingdom, 97 authors and 127 papers. I may mention, however, speaking exclusively of chemistry, —for I have not gone into other sciences,—that, as far as research in Great Britain depends upon our scientific training, our case is much worse than appears from this comparison, because a large proportion of those papers contributed by the United Kingdom were the work of Germans residing in this country."

It is further remarked, in the work before us, that "from a schedule of original researches executed in the laboratory of the Royal College of Chemistry since 1845, and handed in by Dr. Frankland to the Commission, it appears that out of 140 specified researches no less than 70 were made by foreigners! That is to say, Germany not only produces four and a half times as many investigators in chemistry and six times as many researches in a year as we do, but actually produces half the number of researches which in this calculation are credited to this country. I may illustrate the truth of these comparisons by an analysis of the original works published in the West of Europe during a period taken at random, the first fortnight of September, 1873.

* Report of Evidence, p. 371.

"In Physical Sciences I find that thirty-one treatises, almost all of them exhibiting special and original researches, were published. Of these nineteen are German, eight are French, two Italian; while England is represented by Thorpe's "Manual of Quantitative Analysis" and by a popular work on the moon!

"In History the proportion is much more favourable to England, *thanks to the practical endowment of historical research at the Rolls Office.* Of fourteen books four are German, four English, two French, and four belong to various other nations."

This certainly has a most alarming sound, and if we had no other evidence to bring forward we might, on the faith of this alone, lay claim to have proved our proposition that our scientific position, as a nation, is most unsatisfactory. But this is far from all. If we turn from chemistry to other branches of science—to physics, biology, geology, philology—we still find the case in the main unaltered. Our original scientific work is far surpassed in its amount by that of France and Germany, and our competent scientific workers are surpassed in numbers.

Is not the subjoined case truly humiliating? "The fungus which produces the potato-disease is still imperfectly known, and the Royal Agricultural Society—with a laudable desire to remedy that deficiency, in the absence of any laboratory in this country where such investigations are prosecuted—was compelled to have recourse to that of the University of Strasburg, and placed £100 at the disposal of Professor De Bary, in order to have the subject completely investigated."

We come now to a part of the subject which requires a somewhat delicate treatment, but which is far too serious to be passed over. Surely if there is any one thing which we should produce in sufficient amount for our own wants, and which we ought rather to export than import, it is disciplined intellect. To bring into England foreign thinkers, discoverers, improvers, ought to be as superfluous and unremunerative a task as bringing coals to Newcastle. But turning from what should be to what actually is, we must tell a different tale. How many professorships in our universities and colleges are now held by foreigners? How many places of trust in museums, libraries, botanical gardens, and the like, are not in alien hands? How many foreign chemists, and even engineers, are now either in private practice throughout the kingdom or hold the most lucrative and responsible positions in our manufacturing establishments?

On the other hand, is there in France or Germany a single professorial chair—other than of the English language and literature—or any similar position held by a British subject? In India and the Colonies the case is very similar to what it is in the home kingdoms. Now these phenomena are not without cause. We make all due allowance for that decay of national feeling, that morbid cosmopolitanism, which, on the principle that "extremes meet," is generally the outcome of the most intense personal selfishness, and which has been so blatantly preached and so widely practised in England; we know, too, that it flatters the pride of many a *nouveau riche* amongst us to be able to inform his friends that he has German or French chemists in the laboratory of his works, and Italian artists in his designing-room. We are aware that in an unorganised profession like that of chemistry many a stranger, who could not otherwise obtain a foot-hold, contrives to enter by underbidding his native competitors, or even by the unprofessional stratagem of giving his services for a year gratuitously. Nor—low be it spoken!—do we forget that foreign men of science in this country have enjoyed the benefit of exalted patronage. But when all due weight is given to these various considerations we fear the fact remains that French and German men of science are able to take root in this country because they, in many cases, bring to their tasks a more thorough training than do their English rivals. This consideration is to us a very "handwriting on the wall." Is this nation to live, like the dogs, on the intellectual crumbs that fall from the tables of France and Germany? Are we to commit the higher education of our youth, the supervision of the most delicate processes in our manufactures, and the conception and introduction of improvements to aliens, and to allow our own people to subside into the condition of hewers of wood and drawers of water? Is it of any avail that a certain abstract England shall grow more and more rich and prosperous, if it is to be by the degradation of the English people? Can we hope for any length of time to compete with rival nations if we have to borrow from them the intelligence needed to carry on our operations? Alien inventors and managers are in these days no less dangerous than mercenary troops, and if we are content to hire talent from abroad instead of cultivating it at home we are surely preparing our own downfall. Need we recall the fact that the Norman Conquest had been prepared during the reign of Edward the Confessor, by an influx of Norman ecclesiastics, Norman courtiers, and even Norman artizans? Are there not certain phenomena in our day far too closely analogous?

But, returning from this digression, we may surely consider ourselves fully warranted in concluding, from the facts brought forward, that England is not playing a worthy and a creditable part in the increase of human knowledge. We may infer, also, as a corollary, that by such neglect and backwardness her industrial and commercial pre-eminence, her power, and possibly even her very existence as the focus of a mighty Empire, may be gravely endangered.

We have next to enquire what are the causes of the evil which we have just recognised, and which we all, more or less, agree to deplore. We have already stated our opinion that if scientific research does not flourish amongst us as it ought, this is due not to any deficiency in intellect, not to any idiosyncrasy in the national mind, not to indolence or indifference to truth, but simply to the fact that in obedience to an unfortunate complication of circumstances our energies and our intellects have been turned into other channels. Till lately our educational course, both in schools and colleges, has been almost exclusively literary, whilst the subjects popularly known under the name of Science have been totally and contemptuously excluded. With a curious inconsistency they were denounced by men of business as useless, because not conducive to immediate gain; and branded by school-pedants as low, mechanical, and illiberal, because capable of application at all! Even now, in spite of the change which has taken place in public opinion, only ten endowed schools in England give even a poor, pitiful, four hours weekly to the study of science. A contemporary justly remarks—"In a neighbourhood of rural squires and clergy, untempered by a large town's neighbourhood, and unchecked by any man of education and intelligence holding sovereignty by virtue of superior rank and wealth, a school which treads doggedly in the ancient paths will certainly succeed even in second-rate hands, while a school which under superior chieftainship asserts the claims of science, and whose theology is therefore suspicious, will as certainly struggle long for existence, if it does not finally succumb."

Passing from our public schools to the universities, especially the two great and time-honoured institutions of Oxford and Cambridge, we see little on which we may congratulate ourselves. Physical science has, indeed, found a place along with mathematics and classics,—not, however, as a subject to be cultivated and advanced, but merely as a something to be examined in. A more fatal mistake it would be difficult to conceive. "Competitive examinations and original re-

search," as one of the writers of the book before us most aptly remarks, are "incompatible terms."

It may seem a bold assertion, but it is our deliberate opinion that the power of assimilating the views and ideas of others stands in no relation at all to the power of originating anything of value. Receptivity is no sign of fecundity. Nay, the very reverse is often the case—just as it is easier to introduce matter into any vessel the emptier we find it. The man who "crams" easily and quickly, and who shines in competitive examinations, will often be found to have an essentially unproductive mind, poor in resources and barren of suggestions. A. reads every book laid before him by his tutors, and implants its contents in his mind, without the slightest regard to their truth or falsehood, to their value or worthlessness, or to the collateral issues which they raise. B. pursues the exact contrary course; he reads critically; he follows out and tests the author's views, and his mind as he studies is filled with suggestions. We shall of course admit that B. is the true student, and that A. for all higher purposes is merely wasting his time. But the Board of Examiners judge differently; they "pluck" the thoughtful B., and pass the shallow pretentious A.—who certainly differs from a Strasburg goose in being devoid of feathers — in high honours.

But not merely does the system of competitive examinations encourage many of the worst men and reject many of the best. When it once entangles a man endowed with a naturally good intellect, and sincerely anxious for knowledge, it rarely fails to ruin him. The following passage describes the working of this fatal device with no less force than truth :—"The extinction of disinterested study is a necessary consequence of the encouragement of cram. When the best and most receptive years of a man's life have been passed in having the doctrine ground into him that the end of all reading is to cheat the examiner, and that knowledge is valuable only so far as it can be made to tell in an examination, it is hard to see how he can unlearn the teaching he has received, and alter the character that has been formed in him. The grown man is what he has been taught to be, and out of cram may come many pages of examination-answers, or even a fellowship, but not original research, and the love of knowledge for its own sake. The specialist at the universities finds himself a marked man with a wisp of hay upon his horns—he is looked upon with mingled feelings of suspicion and pity. That there can be any knowledge outside the curriculum of the university, or if there is that it is

of any value, is seldom dreamed of. More exclusive than an oligarchy of birth, more sordid than an oligarchy of wealth, we assume that the only subjects worth learning are those in which we examine, and that the worth even of those consists in their being made to 'pay.' Professor Max Müller offered in vain term after term, to read the *Rig-Veda* with any one of the 2400 members of the University of Oxford; none would go to him, since a third hand acquaintance with a few words and forms from that oldest specimen of Aryan literature is sufficient for the schools. The same professor, one of the most fascinating of lecturers, when lecturing on the fascinating subject of comparative mythology, which he has made completely his own, could collect but a miserable fragment of an audience around him, and even of this the larger part consisted of college tutors who intended to retail to their own pupils some of the crumbs which had fallen into their note-books. It is almost impossible to find a majority of fellows in any college willing to give away a single fellowship for a special subject "not recognised in the schools," even when the candidate does not object to be examined; and after this "idle fellowships" are defended on the ground that they encourage study and give an opportunity for learned leisure. But the study and learning that are meant are the study and learning that grow up out of the questions and answers in an examination room. The specialist who pleads in behalf of another kind of learning is considered a fanatic out of harmony with the spirit of our English universities and unappreciative of their merits. "We don't want original researchers," I have not unfrequently heard it said, "but good all-round men"—that is to say, "the best specimens of the crammer who have a smattering of many things, but know nothing well." Did ever system pronounce more emphatically its own condemnation? Surely the humblest "original researcher," the man who discovers only one fact unknown before, is of more value to the world than all the "good all-round men" who have ever lived, or ever will live, till competitive examination is hunted back in contempt and loathing to its native China! "Examination is a bad test," flippantly exclaims a daily paper, "but can you suggest a better?" "Can you," it might be replied, "suggest or conceive of a worse?" But if the reader will have patience to follow us, we will suggest an infinitely better test.

But the system we are condemning ruins not the student but the teacher also. One of the writers in the work which has given rise to the present paper is struck with a remark-

able difference between the Continental and the English colleges. The most brilliant and honoured names in German science, Woehler, the Roses, Mitscherlich, Bunsen, Hofmann, the great Liebig himself—the men who uphold the splendid intellectual reputation of their country, are university professors. In England, on the contrary, the professors, college lecturers, and by whatever other name our university teachers may be known, are not as a class distinguished for original research. Few of them, indeed, could or would even claim to rank with the representative men of British science. This broad distinction, involving as it does the admission that our professorial chairs at present do not form prizes for scientific distinction nor positions where science may be successfully cultivated, is considered by the writer as due to some difference in national character. We think we can give a more correct explanation. The German professor wins his chair by virtue of a reputation based on research. On the amount, the importance, and the success of his subsequent investigations depends the number of students who flock to his lecture hall, and, in the case of the physical sciences, who seek to work under his supervision. As a matter of course, his income, his standing with the Senate and with the Government rise in the same proportion. Decorations, titles of nobility, honours of every kind beckon him on. The most eminent universities outbid each other for his services. With such rewards held out, to be gained by research, and by that means only, is it any wonder that the German professor "works," even if he has not—which is rarely wanting, a sincere and intense love for its own sake? And not only is he diligent in his own person. He selects his more promising students, places his ideas in their hands, assists them with his advice and suggestions, and associates their names with his own in the published results. He thus makes his laboratory, whether physical, chemical, or biological, a very focus of healthy and intense scientific activity. At the risk of somewhat anticipating ourselves, we cannot help here remarking that Germany holds no exclusive patent right for arrangements which not only commend themselves to our common sense, but which have worked so well in actual practice.

The position of an English university professor is almost in all points the opposite. To students, whose great object is not to be initiated into actual research, whether scientific, literary, historical, or theological, not to hear novel and profound ideas fresh from the lips of the master-minds of

the age, but merely to pass certain examinations better than their competitors, what is the use of the eminence of a professor? How many additional students has the reputation of Dr. Max Müller or of Dr. Odling drawn to Oxford, and how much would the number be increased if the merits and the achievements of these two distinguished men were multiplied tenfold? The matter lies in the proverbial nutshell. The English professor does not "work" because he has no adequate motive. Nay, we are far from sure that he has not very direct motives for idleness. In a university where "original researchers" are not wanted, it is very possible that a professor who should succeed in breathing an intense intellectual life into the dry bones around him, would not soon be made to feel extremely uncomfortable and out of place.

But if the universities fail to encourage, or rather if they pointedly discourage, original investigation—there are, it may be said, other establishments founded for the very purpose of training up students for actual scientific research. We will name, for instance, the School of Mines and the Royal College of Chemistry. It is far from our object to bring any charges against either the constitution or the management of these establishments, although we believe that they are guilty of one sin of omission in common with the universities. But we must draw an odious comparison. It is well known that at such establishments as the Polytechnic School of Aix-la-Chapelle, a sound scientific training such as will fit the recipient for a career of research, as well as for the practical application of physics, chemistry, &c., to the arts and manufactures can be had for about £10 to £12 yearly. A similar education in England will be found very much more costly.

But let us suppose that whether at school, at the university, or at the establishments last-mentioned, a young man has contrived to acquire a sound, scientific training. Let him possess the natural qualifications needful, and let his tastes urge him strongly in the direction of scientific inquiry. Shall he—can he—devote his life to the advancement of physics, chemistry, or biology? The answer is too ready. His friends will tell him bluntly, but unfortunately with perfect truth, that "research does not pay." Except, therefore, he possesses independent means, he must abandon his project, or, at the best, effect an unsatisfactory compromise. Of course, if research is to be practically confined to the few, who, with the needful faculties, tastes, and training, combine the advantages of fortune, we need not

wonder that our national amount of scientific work is so limited. What still aggravates the evil is the morbid, wealth-worship dominant in England which actually censures as "idle" the man, however affluent, who does not devote his time to some money-making pursuit. The man content with a competence, in order that he may have full time to devote to some important pursuit, is not a common English phenomenon, and when met with he is more apt to incur ridicule and contempt than respect.

Research, too, whilst it affords its votaries neither wealth nor even the very means of existence, can offer no compensation in the way of honour. Titles and distinctions in this country are very strictly confined to the following classes: —Military and naval men, lawyers, and financiers who "threaten the ministry that they will withdraw their capital from the country unless placed in the social position to which they think themselves entitled." Occasionally an eminent physician or engineer may be rewarded with a knighthood or baronetcy,—more, however, in acknowledgment of the successful routine practice of his profession than of anything done for the extension of human knowledge, —and some few men of science in the strictest term, such as Davy, Brewster, and Murchison, may thus be honoured. Berzelius was ennobled in Sweden, Liebig in Germany, and Cuvier in France, but England refuses to follow this example, and has never, we believe, offered even the lowest grade in the peerage to the most distinguished man of science. Philosophers, we are told, ought to be above such considerations; yet philosophers have very much the same wants as other men. We may safely venture to say that had Newton, Davy, or Faraday been ennobled, the *status* of all scientific men in the kingdom would have been most distinctly raised, and their pursuits would henceforth have met with a much fuller appreciation.

But in addition to all these direct discouraging influences the tendency to scientific research is repressed in an indirect manner, viz., by the circumstances which powerfully tend to draw talent into other channels. The most formidable rival of science in this country is the law which—as regards social position, influence, and emolument—offers rewards far beyond anything that scientific research does or perhaps ever can offer. There are indeed those amongst us who think that we may be content, and accept our bar and bench as an adequate compensation for the want of such a phalanx of scientific worthies as Germany possesses. We cannot adopt this view. We do not see that any conceivable number of

judges and barristers, endowed with even inconceivable ability and integrity, can add to our national resources or enable us to compete to more advantage with foreign rivals. Nor, if we turn from utilitarian considerations to the question of national honour, is the case altered. Strange as it may sound in some quarters, we will venture to say that Faraday alone has done more to raise England in the estimation of the civilised world than have all the most eminent counsel of the day. Hence we do not think that social arrangements which divert talent from the laboratory to the bar are conducive to the national welfare, or that their workings should be witnessed without regret.

Summing up this part of our subject, we may safely venture the opinion that—regard being had to the extremely unfavourable circumstances prevalent—the paucity of scientific research in the United Kingdom cannot excite our wonder, but that there is rather room for surprise and for self-congratulation that under such conditions so much has been accomplished. But there now remains the only useful part of our task—to point out how these circumstances and conditions may be so modified that the scientific intellect of England may meet with fair play. Research does not at present "pay." We must, then, show how it may be made to pay. How is the much needed "endowment of research" to be most satisfactorily effected? Before coming face to face with this question we must first, however, dispose of a few misconceptions which still find a lurking-place in a certain class of minds. Forth steps, for instance, a political economist, of a type very common in Chambers of Commerce, and proceeds to inform us that if research is really valuable it "ought to pay" those who carry it on, and that if it "does not pay" it cannot really be valuable. Our friend evidently forgets that this England of ours not being quite Utopia, things are not always exactly as they "ought" to be. Our worthy objector himself, for instance, "ought" not to adulterate his calicos with China clay, which is yet to be detected in them in large quantity. We are firm believers in the saying of Francis Bacon, that one scientific truth, once established, draws after it "whole squadrons" of utilities and practical operations. But all this does not prove that research, however successful, can be commercially remunerative. A principle established in this century may not find its practical application till the next. Yet are we, on this account, any the less indebted to the discoverer? A scientific investigator is like the first traveller in some unsettled land: he may note the existence of valuable mine-

rals, he may recognise the fertility of the land, but he cannot stop to raise the ore, to clear the forests, and to plant crops. These tasks he leaves to his successors. But is he not all the while to be really credited with the practical benefits springing from his exploration? It is somewhat significant that the olive, the symbolical tree of Minerva, is of very slow growth: he who plants it benefits posterity. Not only do the advantages derived from research often come too late to benefit their author; they are sometimes lost to him from their very width. If we serve an individual man we have some notion where to look for a reward. If we serve a town, a community, or a nation, we may perhaps be told that what is everybody's business is nobody's. But we wish the nation to accept its responsibilities to its benefactors, and therefore to endow research. Nor can we presume to draw a boundary line between useful research and such as seems to be merely curious. The whole history of science warrants us in maintaining that the most abstract truths, to appearance utterly unconnected with the wants of our daily life, may suddenly prove of immense practical importance. Every discoverer of a new fact or a new law has therefore a certain claim, which it is our truest wisdom to honour.

Dismissing, then, this objection, and reminding our friend the economist that the value of anything to the world and its market value are not necessarily proportional, we encounter another protest. The "researcher"—this somewhat unclassical college word is after all useful—we are told may embody his results in a book, and thus make them remunerative. Now in some branches of investigation this is to a certain extent the case. Literary, historical, or especially theological, investigation may thus be rendered self-supporting. In a less degree this holds good with geological and biological research, especially when, as sometimes happens, blended with narratives of adventure, travel, and sport. In such cases a book decidedly scientific in its object may circulate among the outside public and may be "ready at all the libraries," to the benefit of the author. But in physics and chemistry there is no such expedient. A record of discovery in these departments will not make a "readable book." The public at large have not the remotest conception of the amount of labour expended in attaining apparently trifling results. A chemical formula, a mathematical expression—apparently trifling, though really of the utmost value to science—may often embody the whole result of years of anxious, minute, and difficult toil. To give a single instance. A friend of ours was engaged for nearly ten years

with a chemical investigation. He expended during this time in apparatus and materials not less than a thousand pounds sterling. We are not prepared to deny that he elaborated and perfected means of research which will be useful on other occasions, and he "struck the trail" of collateral issues; but what may be called the direct result of his labour was finally expressed in a single line in the "Philosophical Transactions." Surely, then, to bid the chemist or the physicist seek to reimburse himself for his time and trouble by publishing the results of his researches is childish mockery!

Literature, in fact, offers but little scope for a scientific man. Even when a scientific book has to be criticised or a new hypothesis discussed the political and literary organs of the day rarely take the trouble of referring the matter to any specially qualified writer. They know full well that blunders on such subjects are not likely to be detected, however gross, and if detected are not looked upon as disgraceful. It is not long since a morning paper informed the world that all gases, if heated to a point far below redness, exploded, leaving nothing but "a vacuum and a few particles of dust."

The next fallacy we have to encounter is the notion that scientific research may be efficiently carried on by men engaged in professions or in business in the intervals of their ordinary occupation. Some even "out-Herod Herod" to the extent of maintaining that it is better for a scientific man not to have his whole time at his own disposal. We regret to find this absurdity insinuated in a recent biographical work. We do not, of course, deny that certain men have contrived to do valuable work in science, even although trammelled with a trade or a profession. But we hold it to be utterly indisputable—indeed, self-evident—that had their time not been broken into by business they would assuredly have done more. A swift and powerful horse may travel at a certain speed when yoked to a cart, but cut the traces and he will assuredly be able to cover more ground in the same time. It is perfectly true that Daniel Hanbury engaged successfully in chemico-pharmaceutical and botanical researches at the same time that he was at the head of a business establishment. But we must remember that his case was exceptional; the science which he cultivated and the business which he pursued were so closely connected as to be almost identified. But how many branches of science are so intimately connected with any trade or profession? And even in this case, we venture to maintain that Hanbury might and would have done more for his speciality

if he had held, for instance, an *emeritus* professorship. John Dalton maintained himself during a great part of his life by teaching the elements of mathematics. Surely no one can deny that every hour of his time thus occupied was an hour withdrawn from original physical and chemical research. If, indeed, scientific research is best carried on in the intervals of business, it is strangely unlike all other human pursuits. We hesitate, as a rule, to intrust anything of moment to the mere amateur.

We distrust the man who tries to combine even two mechanical trades of a nearly allied nature. We quote the old proverb about the "Jack of all trades and master of none." We admit that "no man can serve two masters." Yet we are some of us unreasonable enough to expect that biology, physics, chemistry may be successfully cultivated, or even *best* cultivated, in the scanty leisure of a man with a body more or less wearied and a mind more or less distracted with other thoughts, cares, and anxieties! Surely at the bottom of all this must lie the notion that science is after all a very paltry matter, not requiring for its successful pursuit any great amount of time, thought, attention, or energy. Yet at the same time, with an inconsistency perfectly refreshing to witness, we often suspect and distrust the professional man, or man of business, if we have reason to believe that he has a literary or scientific "hobby." We fear lest his researches should interfere with the due discharge of his business avocations. Whether his business may not rather interfere with the effective pursuit of his scientific investigation we never ask, such a matter being, we presume, unworthy our consideration. Says the work before us—"I know very well that some who could do admirable service to literature or science, and have accumulated abundant and valuable material, are restrained from making it of general use by publication from the knowledge of the fact that the public would not generally patronise a professional man who, as they say, has a hobby. To some extent, no doubt, this is a reasonable apprehension. I knew a case where a provincial medical man lost a valuable appointment which he had held for some years mainly because he had endeavoured to promote the study of geology in one of the finest geological districts in the whole country." With this quotation we must introduce the reader to an Essay on "Unencumbered Research: a Personal Experience," forming a portion of the work before us. In this paper Mr. H. C. Sorby ably, but very temperately, discusses the mistaken opinion which we are now examining in the light of his own private experience. We

need scarcely say that when so eminent an authority as Mr. Sorby, who can look back on "nearly thirty years of almost uninterrupted practical scientific investigation," analyses, as it were, the processes by which scientific discoveries are made, and shows their general incompatibility with any distracting pursuit or avocation, he is entitled to a respectful hearing. Almost in all points we can endorse his views from our own experience. "For the successful prosecution of original enquiry," he declares, "two of the most essential requisites are abundance of time for continuous and extended experiments, and freedom from all those disturbing cares and engagements which either interrupt the experiments at critical times or so occupy the attention as to prevent the mind from properly digesting the results and deducing from them all the conclusions to which they should lead the investigator. Anyone who has had only a very small amount of experience in original research must have often been struck with the length of time necessary to arrive at a comparatively small result. Many experiments cannot possibly be made to go on in any other than their own slow way. Any attempt to hasten them would probably result in a complete loss of all the time spent over some previous part of the process, or of material which could not be replaced; and even with every care this loss cannot always be avoided."

But it is not merely the absolute amount of time which is the main point. There is here a most important distinction between the student of *books* and the student of *things*, and the difficulty falls upon the latter. The reader, suppose he has only a part of his time at his own disposal, can still leave his books ready, open if he prefers it, and can thus usefully fill up even portions of time of a few minutes duration. Not so the student of things. If he begins an experiment he must be able to attend to it at the precise moment required, or his operations will end merely in disaster. On returning he will find solutions evaporated to dryness, tubes or retorts cracked, temperatures that have exceeded the desired point. The unwatched experiment has led to absolutely nothing but loss of labour and material. Or say he is engaged in biological research—delicate tissues awaiting examination have entered into decomposition, phenomena to be observed have taken place in his absence and have not been recorded. Sometimes the golden moment to be snatched at may depend upon the weather; for the solution of some problem the student may require bright sun-light, or perhaps moon-light. He may wait for days,

perhaps for weeks, for the desired condition. But if his time is not fully his own just at that moment, he may find himself fettered by some professional claim, and the opportunity may slip by.

But this is not all; every one who has been accustomed to watch the operations of his own mind must agree with Mr. Sorby that when grappling with any difficulty, theoretical or practical, the solution cannot be obtained at some given moment, as it were, to order. The man of science seeking the cause of some phenomenon as yet unaccounted for, and, we presume, in a manner strictly analogous; the physician pondering on the origin or the meaning of some strange symptom; the lawyer seeking to fill up some gap in a chain of argument, or the statesman considerating how some difficult negociation is to be carried to a successful issue—cannot sit down and say "Now I will solve the question." The ray of light which makes all plain and intelligible often darts into his mind in what are called idle moments. A casual word uttered in conversation, some trifling fact observed by the wayside and often apparently unconnected with the issue, seems to fire a train, and the mountain of difficulty that lies across our path is shivered to pieces in a moment. This point is ably illustrated by Mr. Sorby from incidents in his own career as an investigator:—"The more I studied the microscopical structure of these cleaved rocks (*i.e.*, those possessing slaty cleavage), the more I was puzzled with the observed facts. One day when quietly walking in my garden and reflecting on things in general, the simplest possible explanation of the whole flashed into my mind. I immediately went into my work-room, mixed some small pieces of coloured paper with wet pipe-clay, and on compressing them in the manner that slate rocks are proved to have been compressed, I found that I obtained a very good representation of the characteristic structure on which their cleavage depends. From that time forward the whole theory of cleavage took a new shape in my mind, and after studying by experiment with the microscope and in the field those facts which this mere hypothesis indicated as important, in a few years I had the satisfaction of observing that it was universally adopted as a perfectly satisfactory explanation of one of the great phenomena of geology."

To take another case—"In studying the structure of meteorites, the evidence of original igneous fusion becomes stronger and stronger, even in the case of the iron masses containing much olivine. This comparatively uniform distribution of such a heavy metal as iron, and of such a

relatively light mineral as olivine, appeared the more to be incompatible with igneous fusion the more one became acquainted with what occurs in our furnaces. In the case of a melted mass of metallic iron and slag the separation is all but complete, and no such thing as an uniform mixture is to be found. For a long time this circumstance remained a puzzle; but in walking out in the country one summer evening, some trivial circumstance, long since forgotten, led me to perceive that the almost immediate separation of the metal and the slag was due to the powerful attraction of our earth, and that in any situation where the force of gravitation was very weak no such separation would occur. The general conclusion then became very simple. Meteoric masses of iron and olivine must have cooled from a state of fusion either as small masses in free space, or near the centre of large planets."

One more instance may be quoted, drawn from a different sphere of research:—"For many years, in common with all who had studied the subject, I had referred the position of the absorption-bands seen in the spectra of different colouring matters to an artificial scale, or, in a few instances, to the principal Fraünhofer lines. One day, however, whilst rambling over the quiet hills of Derbyshire it occurred to me, apparently quite accidentally, that such a system had no physical foundation, and that the time method was to express the position of all parts of the spectrum in terms of the wave-lengths of the light at each part. I at once set to work to contrive the means of doing this conveniently, and it was not long before I succeeded." "It would be easy to multiply such examples and to show the manner in which one train of ideas led to another. Very often the circumstances and train of ideas which led to a discovery were immediately forgotten in the face of the result, like the scaffolding used in the erection of some stately building, and sometimes the circumstances were connected with the main question in a manner so ridiculous that it would be altogether inappropriate to describe them."

From these cases we may surely infer that for the successful prosecution of research abundant and uninterrupted time is one of the main requisites; time not merely for actual thought, but leisure. It will at once strike the reader that in all the cases given by Mr. Sorby the happy thought occurred to him during gentle exercise and when the mind was perfectly at ease. This we can confirm, both from our individual experience and from the statements of friends. But what, then, is the case of a person who has no

leisure, such as the man who endeavours at once to conduct a business or a profession, and to devote himself to scientific research? What if the ideas above mentioned had presented themselves to Mr. Sorby amidst the distractions of business? Might they not have been forgotten before an opportunity occurred of working them out and putting them to the test of experiment? Nay, might not their bearing have altogether escaped notice? One consideration our author appears to have overlooked which very much strengthens his—and our—position. It is this: not all the way-side suggestions which occur to our minds are really valuable. Multitudes must be dismissed as worthless. But everyone alike requires to be duly tested before its value can be known. This of course greatly augments the demands upon the time of the enquirer. But as Mr. Sorby remarks, "if the mind of an investigator is ready to take advantage of every circumstance that may occur, to press forward his enquiry in the line of truth, the removal of the most formidable difficulties is a mere question of time." In summing up the question he adds—in our opinion most truly—"One thing, however, is clear and cannot be denied; no one can even remotely approach to this mode of life and continuous observation whose mind is constantly engrossed with other cares—whose thoughts are necessarily directed to the consideration of how he can provide for the needs of each coming day, or how he can extricate himself from or avoid pecuniary embarrassment. Whatever the experience of others may lead them to think, mine has been amply sufficient to convince me that I never could have done what I have been able to do if it had been necessary for me to attend to any business or profession as a means of support. Though I wish I had been able to do more, yet if I had been interrupted by the cares of practical life I should certainly have done far less, and in all probability the general quality of the work would have been deteriorated to a still greater extent."

If, therefore, Mr. Sorby had not been in an independent position his contributions to science—and they are not small—would have been diminished or lost altogether. But how many men in easy circumstances possess the natural abilities and tastes, or have received the training, which incline and qualify them for a career of research? There is a proverb which tells us that "necessity is the mother of invention." In so far that when some particular public want has made itself felt and has attracted the attention of many minds, it will probably be met this adage is perfectly true. But if it

be understood to mean, as we fear is sometimes the case, that personal want or distress may drive a man to scientific investigation who, if in better circumstances, would remain idle, we must with our whole soul protest against an absurdity so glaring. Men in distress naturally turn their attention to something immediately remunerative, which the most successful scientific investigation can rarely if ever be.

Another consideration here suggests itself. In the two last centuries there were in various parts of England rich coal beds lying near the surface and capable of being worked at a comparatively small outlay. Now, these easily accessible beds being exhausted the miner is compelled to sink deeper, and to work at a greater cost. Not otherwise is it in science. The facts and truths that lay near at hand have been already gathered in. We have now to go farther afield, to use costlier because rarer materials, to correct the approximate determinations of our predecessors, and in so doing to employ expensive instruments of precision. Little could be done in these days with apparatus such as that used by Dalton or by Davy in the outset of his career. Hence it has become more difficult for a poor man, unaided, to win his way to eminence.

With Mr. Sorby we admit that there are here and there exceptional positions in some of our manufacturing establishments where at any rate chemical and physical research may be carried on. But such opportunities are rare indeed. A "works' chemist" generally finds his whole time absorbed in routine duties, and is merely tantalised by opportunities of which he may not avail himself, phenomena which he is debarred from examining, and suggestions which he must suppress. We have known a "works' chemist" grossly insulted by the head of the establishment for having called his attention in private to an abnormal amount of arsenic in a sample of pyrites. "Determine nothing in future in pyrites but sulphur, copper, and silver" was the command. For sciences other than chemistry or physics manufactures afford little scope.

We must therefore pronounce it a delusion to expect that research can, to any worthy extent, be carried on "in the intervals of business," and must return to the conclusion that the nation which wants it and must have it must be prepared to endow it. But how?

Our first reply to this question brings us in immediate contest with the vexed topic of "university reform," and so lands us in a region of political and theological quarrels. When men cast about for funds for the endowment of re-

search they remember naturally that the nett income of Oxford, taking the university and the colleges together, amounts to the startling sum of £400,000 a year. Of this income £91,545 a year are absorbed by the "Fellowships," which at Cambridge account for £92,820 annually. "These Fellowships at present are mere sinecures, with not even ostensible duties attached to their possession. The value of a Fellowship at each University may be taken to vary between £200 to £300 per annum, paid, for the most part, in the form of a life-pension, terminable on marriage. These prizes are awarded in open competition, either by special examination, or as an indirect result of a high place in the public examinations." Surely, then, these Fellowships are the very thing we are in search of for the endowment of research. But the moment this change is mooted, there rises from amongst those who think, or at least call, themselves Conservatives a hostile hum made up of the words "confiscation, spoliation, violation, communism, revolution, confusion, pious founders," and the like; and amidst the sea of sound we catch exhortations to this effect:—"Our Universities are educational or tuitional, not investigational. If you want places for the advance and increase of human knowledge, found and endow them yourselves!"

At the same time Liberalism, so-called, stands chiefly mute, fearing lest its Chinese idol, competitive examination, should be overthrown, and his joss-house burnt in the struggle. A few only are found who declare boldly that, *per fas aut nefas*, the dead hand of the pious founder notwithstanding, such sums must be seized and applied to a more useful purpose. But what if both parties, on closer examination, are found to be in the wrong? What if the skeleton-hand of the pious founder, instead of being held up in menace to bar our way, is found to be beckoning us on? What if we require to effect the needed reforms, no confiscation, but merely a carrying out of the original design of old worthies? Contrary to the belief alike of their friends and their foes, our great national universities are not exclusively and intentionally mere places of education. It was never the intention of the founders, whether of the universities or of their individual colleges, to make them mere places where a set of young men were to be crammed for examination. The advancement of knowledge was an end constantly kept in view. The Fellows were supposed to be a body of men giving up the whole of their time to study and research. Thus, in Merton College, the Fellows "are to employ themselves in the study of Arts, of Philosophy, the Canons, or

Theology, but the majority are to continue in the liberal Arts and Philosophy." In the statutes of New College, founded by William of Wykeham, under charter from Richard II., the following passage occurs :—

"We desire, moreover, that our scholars (the original name of the Fellows) occupied in diverse sciences and faculties may, by their intercourse with each other, learn something new every day, and by continual advance become better and better, that the spirit of the whole multitude tending to the same end may be one, and that through the Divine mercy our colleges endowed with and supported by men of so many sciences may the more firmly and securely abide and continue for ever in the beauty of peace."

In All Souls College there were to be forty Scholars or Fellows, who were to study without intermission. Of these twenty-four were to study the Arts and Philosophy, or Theology.

In New College it is expressly stipulated that two of the Fellows might devote themselves to Astronomy, and two to Medicine.

In Magdalen College, of the seventy "poor and indigent Scholars" forty, called "Fellows," were to study Theology and Moral and Natural Philosophy.

Many more such extracts from the statutes of the colleges might be given, all tending to prove that, in the words of the Report,* "The duty of the Fellows, as such, was, as we shall show more at length hereafter, *not to teach, but to learn;* hence the earliest name of this class . . . . . scholares. (P. 134.)

"The most important object of colleges was, as Blackstone states, '*ad studendum.*' To receive and not to give instruction was the business of the Fellows of colleges. The founder of Queen's has expressly declared that he intends by his benefaction to relieve his fellows from the necessity of teaching. In all colleges, even in those which aimed at supplying instruction to the university, the great majority of the Fellows were intended to devote their life to study, and not to engage in teaching, either in the college or in the university."†

Summing up the evidence thus collected from the statutes themselves and from the Commissioners' Report, the essayist declares that the "*fellowship system originated in a desire to promote study and not to promote teaching.*" Their purpose was

* First University Commission of 1852.
† Report, p. 140.

not educational, tuitional, but investigational. "It is certain that the present generation of Fellows, whether tutors or non-residents, are alike wrongful occupants of their comfortable positions, and usurpers when tried at the bar of history."

Claiming, then, as we do, these positions for men capable of and devoted to research, we are assuredly not revolutionists, spoliators, and confiscators, but upholders of the rights of property and of the sacredness of bequests. Old precedent speaks not against us but for us. What we urge is not an innovation. "Up to the close of the last century Oxford and Cambridge always took the lead of the nation in all intellectual matters. Their pre-eminence was due not to the efficiency of their instruction, but to the presence of the few industrious holders of sinecure appointments. . . . . It is more to the purpose to draw attention to the neglected fact that even in the physical sciences have college fellowships proved themselves in the past to be not unkindly nurses.

"It can hardly have escaped the attention of the reader that in the extracts from the statutes given above, Medicine, Natural Philosophy, and Astronomy hold a considerable place. It is indeed only a deplorable ignorance of history which induces people to regard the present revival of scientific studies at the Universities as an absolute innovation, characteristic of the latter part of the nineteenth century. William of Wykeham contemplated that two of his Fellows should apply themselves to Medicine and two to Astronomy; while William of Waynflete, his worthy imitator, named Natural Philosophy as one of the three chief objects of study at Magdalen. Nor has the actual fruit been altogether unworthy of these liberal designs. It was Thomas Linacre, a Fellow of All Souls, a man who combined to a rare degree classical taste with scientific erudition, that first raised the practice of medicine to an honourable status in this country, and induced Cardinal Wolsey to found the Royal College of Physicians in London at the commencement of the sixteenth century. Of that learned body Linacre was the first President. He was succeeded in the chair by Dr. Caius, himself a Fellow of Gonville Hall at Cambridge, which he subsequently erected into the College which bears his joint name. In later days Sydenham, the father of the scientific study of medicine, was a student of Christ Church, and testified to his intimate connection with that corporation by bequeathing to it his valuable library. It ought, also, never to be forgotten that it was among a

select band of Oxford Fellows and their friends that the Royal Society first saw the light. In Astronomy, besides the peerless name of Newton, who was emphatically an academical recluse, the lists of eminent professors who were not teachers, both at Oxford and Cambridge, might be quoted, of whom Bradley is only one among many. These examples have not been discovered by means of any remote enquiry, but are merely jotted down as they suggest themselves to one who has no books of reference at hand. Their number might be indefinitely extended, but as it is they are more than sufficient to demonstrate that in every branch of knowledge Oxford and Cambridge have fairly held their own; and that *endowed sinecures did not turn out a failure till it was announced, with quasi-Parliamentary sanction, that the traditional duty of study was no longer to be expected from the Fellows.*"

We have merely, then, to restore the universities to something of their old condition, though on the wider and deeper scale suitable to the modern position of science and learning. We must cast aside, on the one hand, the corruptions and neglect of the eighteenth century, and on the other the pseudo-reforms of the nineteenth. All this can be done by the enactment of a new principle in the attainment of fellowships, and indeed of all high honours; instead of election with its jobbery and favouritism and corruption, or competitive examination with its cram and quackery and superficiality, and hostility to all true intellectual life, let us have research as the only test. One of the writers of the work before us gives it as his opinion—"Only when a man has finished his training and proved his competence by some published investigation should he be appointed to a fellowship." Although speaking of theological research he thus advocates our scheme, since the principle is one and the same. We are far from claiming the whole of the fellowships for physical science. We are pleading the cause of investigation in philology, in history, in theology, as well as in biology, chemistry, physics, astronomy, or geology. Let there be a fair division. Then let every candidate for a fellowship be bound to produce some piece of original research in the science or branch of erudition to which he intends to devote his life. Of course only students who have duly passed through their proper curriculum, and who have proved that they have reached a certain preliminary standard, will be allowed to submit to this new trial. It will, however, be much less of a competition than the present system, since a great number of minds—those, indeed, of the class who now often make the greatest figure—will find themselves at

once disqualified. The successful candidate will then hold his life-pension, *on condition of work*, a minimum scale being probably decided on at the outset. But as no one can on this system possibly attain a fellowship who has not proved himself able and willing to enter upon original research, there will be, we think, little fear of idleness. As for the unsuccessful candidates—those whose attempts at original research are found inadequate—they will not, like the defeated, or, in fact, like the successful candidates in our competitive examinations, be useless creatures. The training they have received will make them very valuable in certain positions. If they have selected dynamics, physics, or chemistry, an honourable career in the arts and manufactures may still be open to them. But this one alteration will pass through the entire university, and operate a beneficial change in every department. The students will flock to those professors who will be best qualified to initiate them into the mysteries of actual research, and Oxford and Cambridge will become once more centres of earnest progressive scientific activity, regularly pouring forth their streams of discovery. Such establishments as Owens College, the Yorkshire College of Science, those of Birmingham, Bristol, and Newcastle, all which we hope soon to congratulate as formally constituted universities, will doubtless, in course of time, have their fellowships to offer on similar conditions. It is an encouraging fact that wealthy and benevolent individuals are now evidently more disposed to give or to bequeath large sums for the encouragement of higher education,—surely a more useful employment of their funds than the old custom of founding charity-schools, where some score or two of unfortunate urchins were dressed up in absurd costumes, to be the sport and the butt of the neighbourhood. A few fellowships, open only to the authors of the best piece of original research, say in biology or chemistry, would be invaluable appendages to these our new schools of science. Even where no fellowships, *emeritus* professorships, or other endowments exist, research, if not actually subsidised or endowed, is easily capable of promotion. In conferring degrees the production of some original investigation might be advantageously demanded from the candidate, either in addition to the present examinations, or, perhaps still better, in lieu thereof. Let us, for instance, take the degree of Doctor of Science, as conferred by the University of London. Suppose that a Bachelor of Science, instead of submitting to the usual examination, had the option of earning the higher step by the producing of an original paper in connec-

tion with any branch of science in the course. It would be a healthy change if, instead of speaking of a student "reading" for this or the other degree, we could ever come to speak of his "working" for it.

We have spoken of the system of competitive examinations as a Chinese invention; but it has also another root. As one of the essayists before us observes—"The introduction of the system of emulation into the higher studies is historically traceable to the Jesuits. The adoption of the principle of perpetual supervision of repeated examinations, of weekly exercises, produced marvellous effects in the Jesuit colleges. It was not till the first half of the eighteenth century that opinion began to turn. It was then found that beneath this brilliant show of college exercises and prizes was concealed a starved and shrivelled understanding. The work done in class was pattern-work, but the pupil whom the institution turned out was a washed-out, frivolous, superficial being." Whilst fully recognising that such must be the result in every seat of learning where examinations are made a leading feature,* we believe that the Jesuits introduced into their system one arrangement in which English colleges are exceptionally deficient. They recognised the extreme diversity of individual tastes and abilities, and the no less indisputable truth that there are many distinct methods for effecting the discipline of the intellect. They consequently did not force every pupil through one undeviating course, but gave him his choice between equivalent studies.

In this country science has been hardly dealt with, both by its friends and its enemies. The former have urged its claims rather on the score of utilitarian results than as a means of mental training; and the latter, without a particle of evidence, have assumed its inferiority. In Germany specialism begins at the very gate of the university, and the student selects at once his faculty. That this arrangement facilitates profound scientific research we feel certain.

In the following passage Prof. Max Müller, substantially and most ably, pleads for the kind of university reform which we have been proposing:—"Whatever fellowships were intended to be, they were never intended to be mere

* We have known a college where examinations were held at quite irregular intervals, without even an hour's previous warning. This stratagem rendered cram, and indeed every form of special preparation, impossible, and gave the heads of the establishment a far clearer insight into the attainments and powers of their students than could be obtained by any other form of examination.

sinecures, as most of them are at present. It is a national blessing that the two ancient universities of England should have saved such large funds from the shipwreck that swallowed up the corporate funds of the Continental universities ; but, in order to secure their safety for the future, it is absolutely necessary that these funds should be utilised again for the advancement of learning. Why should not a fellowship be made into a career for life, beginning with little, but rising like the incomes of other professions? Why should the grotesque condition of celibacy be imposed upon a fellowship instead of the really salutary condition of no work no pay? Why should not some special literary or scientific work be assigned to each Fellow, whether resident at Oxford or sent abroad on scientific missions? Why, instead of having fifty young men scattered about in England, should we not have ten of the best workers in every branch of human knowledge resident at Oxford, whether as teachers, guides, or examples? All this might be done to-morrow without any injury to anybody, and with every chance of producing results of the greatest value to the universities, the country, and to the world at large."*

Turning from university reform considered in the interests of "research," we have yet to speak of certain establishments, national and public, in which the services of scientific men are required. There are museums, observatories, botanical gardens; there are geological surveys and occasionally exploring expeditions. In all these there are positions which afford scope, greater or less, for research. But how are they filled? Formerly, we believe, candidates were selected by interest. Now competitive examination reigns. Would it not be better, except in case of men who have already "won their spurs," to adopt our test, and thus secure the right man in the right place?

But we cannot stop here; even when we have converted our universities, present or nascent, into centres of investigation and made their highest honours and their more tangible rewards accessible solely by the way of successful research, something more is wanting. It does not suit the national mind to be tied down too rigidly. We must make provision, therefore, for the encouragement of research among those who may have aimed at a college fellowship, but whose performances have not been found of sufficient value; among such as, though educated at one of the universities, have not found it consistent with their plans

* Chips from a German Workshop, iv., 4—10.

and duties to offer themselves as candidates for fellowships or professorships; among British subjects educated abroad, and, lastly, upon the important class who have qualified themselves for research by private study without having ever been connected with any college or public institution. Nay, even those who have won a fellowship and hold it, according to our proposal, on condition of not remaining idle, would work all the more zealously if in prospect of further rewards consequent upon success.

We suggest, therefore, that some learned body or bodies of high position should have the duty of examining all papers sent in and should be enabled to reward the authors of such as are found of value with a round sum of money proportionate to their merits. The details of such a scheme will of course require long and careful consideration, but we submit that the most suitable body to be entrusted with a task so responsible and so honourable would be the Royal Society. From its midst it would be easy to select a committee fully competent to pronounce on the value of any paper placed in their hands and no less above suspicion of foul play. As to the requisite funds, if Henry Cavendish had only carried out his original plan of bequeathing his large portion to the Royal Society for the furtherance of research no difficulty would have existed. As, however, he unfortunately altered his intention, the needful resources would have to be provided either by a Parliamentary vote or by private bequests, donations, and subscriptions. Those who mistrust the impartiality of the Royal Society are reminded that the application of funds obtained in either of these ways would be jealously scrutinised, and that any suspicion, even of favouritism, would at once lead to a rigid investigation. The papers sent in might, as in the case of tenders for a contract, be signed, not with the author's name, but with some motto or cypher. As to the scale of reward, we should suggest that a paper pronounced worthy of insertion in the "Philosophical Transactions," should entitle its author to—say £500.

As an alternative proposal, the task of considering and rewarding original investigation, instead of being committed exclusively to one body, might be distributed among several, as, *e.g.*, the British Association, which does already assist research by sums voted as subsidies to men of science engaged with certain specified questions. The Astronomical, Linnean, Geological, and other Societies, might also be proposed as qualified and entitled to a share of this duty. To us, however, there appear weighty objections to any division.

There might occur conflicts of jurisdiction, and widely different estimates of the value of work done. Two papers substantially identical might be sent, by one and the same author, to two different Societies. A dissertation rejected by one authority might be entertained by another, to the occasioning of no little scandal. Subdivision could, moreover, hardly fail to weaken the sense of responsibility. As, moreover, the Royal Society rarely fails to include all the most eminent men in every department of science, it cannot be urged that any paper could find in any other Society a more competent body of judges.

In submitting these schemes for the promotion of research in England, or rather amongst the English nation, we are far from conceiving them as incapable of improvement. All we desire is that our country shall not fall short of any foreign land in the value and the extent of its contributions to science; and if this end can be more effectually promoted in any other manner than in those we have suggested, we shall be the first to abandon our own views and to labour for the adoption of the more excellent way.

Mr. Dyer says—"Everywhere the harvest of new knowledge is maturing and ripening. England alone is loth to send labourers to reap it."

V. THE BOOK OF THE BALANCE OF WISDOM.

By H. Carrington Bolton, Ph.D.

So rapid are the strides made by science in this progressive age, and so boundless is its range, that those who view its career from without find great difficulty in following its diverse and intricate pathways, while those who have secured a footing within the mysterious domain and are free to journey on the same road are often quite unable to keep pace with its fleet movements and would fain retire from the unequal contest. It is not surprising, then, that those actually contributing to the advancement of science, pressing eagerly upward and onward, should neglect to look back upon the labours of those who precede them, and should sometimes lose sight of the obligations which science owes to forgotten generations.

Could the wisdom of the world concealed in the silent unwritten history of past ages be divulged by a miracle of

revelation, how startling and interesting would be the wonderful disclosure! Imagination fails to conceive of the possible social and scientific status of the present era, had the named and the unnamed "lost arts" been preserved through all time, and had the experience of the human race been uninterruptedly handed down to the existing generation.

The legitimate feeling of exultation and satisfaction enjoyed by oriental scholars when years of painstaking research amid the musty records of the past are at length rewarded by a literary and historical discovery of importance, seems to us comparable to the pleasurable excitement experienced by the scientists whose investigations of nature are crowned by the determination of a new species or the establishment of a new law. In this respect, the Egyptologist and the Naturalist, the student of ancient history and the student of modern chemistry, have a common purpose, each in his own sphere seeking the truth.

Original investigations in the field of history are, however, vouchsafed only to those profound linguists whose erudition and skill in deciphering semi-obliterated cryptographs have been the fruit of a lifetime's laborious study; and these, absorbed in their study of the ancient, too often neglect to compare the wisdom of early times with the progress of modern scientific truths, and fail to appreciate the points most valuable to the student of science. Hence the history of science yet remains to be written.

This suggestion may be met with references to the works of the *savants* who, especially in the preceding century, devoted much to the elaboration of historical treatises in their several departments of science; but these are few in number, and, as we believe, merely sketch the outlines, the details of which will yet be supplied by some mighty genius at once a linguist, an archæologist, and a scientist.

Meanwhile, in default of the erudition which alone allows of critical examination of monumental inscriptions, papyri, and original manuscripts, the humble searcher after knowledge must be content to study available translations, notes, and criticisms provided by oriental scholars, and to bring into prominence such materials as his imperfect powers can command.

Bearing in mind the great obligations which the exact sciences owe to the Arabian philosophers of the Middle Ages, it is not surprising that such of their works as are made available by the translations of linguists afford abundant and rich sources of information to the student of the History

of Science. The work named at the beginning of this article forms a remarkable contribution to the early history of the determination of specific gravities. This "Book of the Balance of Wisdom" is an Arabic treatise on the water-balance, written in the twelfth century, for an account of which the historian of science is indebted to the Chevalier N. Khanikoff, some time Russian Consul-General at Tabriz, an important city of Northern Persia. M. Khanikoff having obtained access, in some manner not explained, to a manuscript copy of the Arabic work, translated into the French language copious extracts, and prepared an analysis of its contents: these data, together with a transcript of the original Arabic version, he communicated to the American Oriental Society. The Society's Committee of Publication, in preparing the Russian Consul's work for their Journal, translated his notes into English, re-translated the Arabic extracts, and added their own valuable comments. The completed article is found in the sixth volume of the "Journal of the American Oriental Society," pp. 1 to 128, published in 1859.

The "Book of the Balance of Wisdom" treats exclusively of the balance, and of the results obtainable by this instrument, which has given to modern science so many beautiful discoveries. Its exposition of the principles of the centres of gravity, of researches into the specific gravity of metals, precious stones, and liquids, shows these Orientals to have attained to experimentation, a step in the progressive knowledge of physical truths entirely unknown to the ancients. Before offering such citations from this work as may seem necessary to establish this proposition, we will endeavour to answer two questions which naturally suggest themselves to our readers. What is the date of the original manuscript, and who was its author? Most happily both queries admit of satisfactory replies, based on internal evidence.

The dedication of the work proves it to have been composed at the Court of the Saljuke Sultan Sanjar, who reigned over a large part of the ancient Caliphate of Bagdad from A.D. 1117 to 1157. In this introduction the author appeals to this potentate in the following fulsome expressions of homage characteristic of the Orientals:—

"Most magnificent Sultan, the exalted Shah of Shahs, the king of subject nations, the chief of the Sultans of the world, the Sultan of God's earth . . . the shelter of Islamism and of Muslims, the arm of victorious power . . . Prince of Believers—may God perpetuate his reign and double his power! For his felicity is the illuminating sun of the world, and his justice its vivifying breath."

And immediately following this passage, occurs mention of the date:

"I sought assistance from his beams of light irradiating all quarters of the world, and was thereby guided to the extent of my power of accomplishment in this work, and composed a Book on the Balance of Wisdom for his high treasury, during the months of the year 515 of the Hegira of our Elect Prophet Mohammed—may the benedictions of God rest upon him and his family, and may he have peace!"

This proves the treatise to have been written in the years 1121-1122 A.D. At this period of the world's history we find Arab philosophers cultivating literature and science, while the rest of Europe was just immerging from intellectual darkness. The religious world had scarce recovered from the intense excitement aroused by Peter the Hermit and his followers, who led the tumultuous rabble 600,000 strong towards the Holy City; and priests, knights, and peasants were preparing with frantic zeal for a Second Crusade. Abélard and Héloise were names which afforded endless discussions in the cloister and on the hearth. Science was at a low ebb, a century elapsed before Albertus Magnus and Roger Bacon exerted their influence; and scholastic philosophy, attaining its loftiest height, swayed the intellects of the age.

The authorship of the Book of the Balance of Wisdom is easily determined by the fortunate circumstance that the author names himself several times, "but in so modest a manner as scarcely to attract attention; instead of heralding himself at once, in his first words, after the usual expressions of religious faith, as Arab authors are wont to do, he begins his treatise by discoursing on the general idea of the balance" and then simply remarks: "*Says al-Khazini, after speaking of the balance in general,*" and proceeds to enumerate the advantages of the balance which he is about to describe. Two other passages in the extracts furnished by M. Khanikoff satisfy the Oriental scholars who have examined them that the author is the self-named al-Khazini.

Attempts to identify al-Khazini with individuals of historical fame have given rise to differences of opinion, but the weight of evidence is in favour of regarding him as the same with Alhazen, the Arab optician and physiologist.

Alhazen seems to have been a native of Persia, and to have resided in Spain and Egypt, but of his biography little is known. He is especially distinguished for his demonstration of the theory of vision, showing that the rays of light are reflected from external objects to the eye, and do not

issue forth from the eye to impinge on external things, as up to his time had been taught.

This explanation, moreover, was not based on mere hypothesis, but was the result of anatomical investigations as well as of mathematical discussions.

Alhazen also explained the astronomical refraction of light, its dependence on the variation of the density of the media traversed, and its influence in producing the phenomenon of twilight. In the discussion of all these problems he evinced true scientific greatness. He favoured the theory of the progressive development of animal forms, anticipating a doctrine but newly obtaining acceptance. Dr. J. W. Draper,* who has been our guide in this connection, says of Alhazen: "Though more than seven centuries part him from our times, the physiologists of this age may accept him as their compeer.

The name al-Khazini signifies "related to the treasurer," which accords with his statement that the work was composed for the royal treasury.

The Book of the Balance of Wisdom begins with a dedication to God "the compassionate, the merciful," and a pious statement of the author's religious faith. An introduction, divided into eight sections, then follows; in the first section, the advantages and uses of the Balance are enumerated in this language:

"These advantages are:—1, exactness in weighing; this balance shows variations to the extent of a mithkal,† or of a grain, although the entire weight is a thousand mithkals, provided the maker has a delicate hand, attends to the minute details of the mechanism, and understands it; 2, that it distinguishes pure metal from its counterfeit, each being recognised by itself without any refining; 3, that it leads to a knowledge of the constituents of a metallic body without separation one from another . . . . . .; 4, that it shows the superiority in weight of one of two metals over the other in water, when their weight in air is the same, and reversely . . . . . .; 5, that it makes the substance of the thing weighed to be known by its weight . . . .; 6, ...; 7, the gain above all others——that it enables

* Hist. Int. Devel. Europe, page 360.

† We connot here undertake to discuss the ancient Arabic system of weights, but merely state that while authorities differ, M. Khanikoff, after a careful examination and comparison of modern and ancient standards of weights in Georgia, Daghistan, and elsewhere, where Arabic customs have suffered little change, assigns to the mithkal the value of 4·5 grammes. The mithkal, according to Abu-r-Raihan (quoted below) equals 6 daniks; 1 danik = 4 tassujs; 1 tassuj = 4 grains; and 1 grain = 4 barley-corns.

one to know what is a genuine precious stone, such as a hyacinth, or ruby, or emerald, or a fine pearl; for it truly discriminates between these and their imitations or similitudes in colour, made to deceive."

Then follows the theory of the water balance; and in the fourth section some account of its early history and the well-known narrative of King Hiero's crown.

"It is said that the [Greek] philosophers were first led to think of setting up this balance, and moved thereto, by the book of Menelaus* addressed to Domitian, in which he says: O King, there was brought one day to Hiero, King of Sicily, a crown of great price, presented to him on the part of several provinces, which was strongly made and of solid workmanship. Now it occurred to Hiero that this crown was not of pure gold, but alloyed with some silver; so he inquired into the matter of the crown, and clearly made out that it was composed of gold and silver together. He therefore wished to ascertain the proportion of each metal contained in it, while at the same time, he was averse to breaking the crown on account of its solid workmanship. So he questioned the geometricians and mechanicians on the subject, but no one sufficiently skilled was found among them, except Archimedes the geometrician, one of the courtiers of Hiero. Accordingly he devised a piece of mechanism which, by delicate contrivance, enabled him to inform King Hiero how much gold and how much silver was in the crown, while yet it retained its form. This was before the time of Alexander. Afterwards Menelaus [himself] thought about the water-balance and brought out certain universal arithmetical methods to be applied to it; and there exists a treatise by him on the subject. It was then four hundred years after Alexander."

Al-Khazini takes pains in this sketch of the early history of specific gravity, to establish dates by reference to contemporaneous individuals, but his chronology is evidently at fault. The Hiero alluded to is Hiero II., who died 216 B.C., while Alexander the Great lived more than a century earlier (356 B.C.—323 B.C.)

This Arabic version of the anecdote of Hiero's crown lacks the piquancy and interest of the narrative as originally given by Vitruvius, and is moreover not an accurate transcription; the words "devised a piece of mechanism" convey the impression that Archimedes constructed some peculiar form of apparatus with which to solve the problem. Indeed this

* A celebrated mathematician who lived in the reign of Trojan, 98—117 A.D., and author of a treatise on Spherical Geometry.

story, familiar as it is, is not unfrequently erroneously related, even as al-Khazini himself has done, and compilers of text-books of natural philosophy, content to copy from each other instead of seeking the original sources, transmit the errors of detail.

These reflections are in part suggested by a singular construction given to the narrative in a recent and really excellent text-book on the "History of Natural Science," in which the authoress commits the remarkable anachronism of representing (in a woodcut) the crown and the metallic masses suspended in water from *spring-balances* of modern appearance and construction.

To dispel any lingering ideas of this character we here revive the passage in Vitruvius,* "*De Architectura*," the original source of the narrative, and in which it appears that the "greatest geometer of antiquity" arrived at his results by a comparison of unequal volumes of water obtained by displacement, and not by direct weighings in that liquid.

"Book IX. Chapter 3. *Of the method of detecting silver when mixed with gold.*"

"Though Archimedes discovered many curious matters which evince great intelligence, that which I am about to mention is the most extraordinary. Hiero, when he obtained the royal power in Syracuse, having, on the fortunate turn of his affairs, decreed a votive crown of gold to be placed in a certain temple to the immortal gods, commanded it to be made of great value, and assigned an appropriate weight of gold to the manufacturer. He, in due time, presented the work to the King, beautifully wrought, and the weight appeared to correspond with that of the gold which had been assigned for it. But a report having been circulated that some of the gold had been abstracted, and that the deficiency thus caused had been supplied with silver, Hiero was indignant at the fraud, and unacquainted with the method by which the theft might be detected, requested Archimedes would undertake to give it his attention.

"Charged with this commission, he, by chance, went to a bath, and being in the vessel, perceived that as his body became immersed, the water ran out of the vessel. Whence, catching at the method to be adopted for the solution of the proposition, he immediately followed it up, leaped out of the

* Marcus Vitruvius Pollio, a distinguished Roman architect and author, served as military engineer under Julius Cæsar in Africa, B.C. 46. His work, "De Architectura" (written in his old age), comprising ten books, is the only ancient treatise on the subject extant. Of Vitruvius's biography very little is known.

vessel in joy, and returning home naked, cried out with a loud voice that he had found that of which he was in search, for, he continued exclaiming in Greek, Ευρηκα (I have found it out).

"After this he is said to have taken two masses, each of a weight equal to that of the crown, one of them of gold and the other of silver. Having prepared them he filled a large vase with water up to the brim, wherein he placed the mass of silver, which caused as much water to run out as was equal to the bulk thereof. The mass being then taken out, he poured in by measure as much water as was required to fill the vase once more to the brim. By these means he found what quantity of water was equal to a certain weight of silver. He then placed the mass of gold in the vessel, and on taking it out, found that the water which ran over was lessened, because the magnitude of the gold mass was smaller than that containing the same weight of silver.

"After again filling the vase by measure, he put the crown itself in, and discovered that more water ran over than with the mass of gold, that was equal to it in weight; and thus from the superfluous quantity of water carried over the brim by the immersion of the crown, more than that displaced by the mass, he found by calculation, the quantity of silver mixed with the gold and made manifest the fraud of the manufacturer." *

Continuing the sketch of the history of the water-balance given by al-Khazini in the fourth section of the introduction, we find references to several Arabian philosophers, among them the celebrated physician Avicenna (Ibn Sina) who "distinguished [the components of] a compound scientifically and exactly," and Abu-r-Raihan "who took observations on the relations of [different] metallic bodies and precious stones, one to another, as indicated by this balance."

Al-Khazini also states that the instrument in question was called "the Physical Balance" by Mohammed Bin Zakariya of Rai, and it was named "the Balance of Wisdom," by the "eminent teacher Abu-Hatim al-Muzaffar Bin Ismael of Isfazar."

Abu-r-Raihan alluded to above is often quoted by al-Khazini and deserves our attention. He was a distinguished Arabian astronomer, born about 970, and died 1038 A.D. He was a member of the Society of Savans founded in the capital of Kharizm, and of which the eminent physician Avicenna

* The Architecture of Marcus Vitruvius Pollio, in ten books. Translated from the Latin by Joseph Gwilt, London, 1826, pp. 264, 265.

was a shining light. He was the author of a number of works on astronomy, cosmography, and physics, one of which entitled "The Book of the Best Things for the Knowledge of Mineral Substances," and contained in the Ayin-Akbari, or Institutes of the Emperor Akbar, treats of the specific gravity of bodies and of hydrostatic methods for determining them. It is this work to which al-Khazini refers. A review of Abu-r-Raihan's treatise has been published by Mr. J. J. Clément-Mullet, under the title, "Recherches sur l'Histoire Naturelle et la Physique chez les Arabes." * Since this Arabic manuscript is probably the most ancient work extant which systematically treats of specific gravities, we make another digression and give a brief synopsis of its contents.

It contains theoretical explanations of the origin and formation of mineral bodies founded on the views of the Greeks, and particularly on those of Aristotle. According to these views, the variety of the weights of bodies depends upon the dry and the moist exhalation from them; air and water are the elements of these vapours, air giving the lightness and water the heaviness. The author then proceeds to describe methods for determining the specific gravity of bodies in the following words:—

"Scientific men determine by means of water the measure of these differences in weights. They prepare a vessel filled with water in which they introduce 100 mithkals of each of the metals; the quantity of water thrown out by each gives the difference in volume and weight, that one which displaces the largest bulk of water has consequently the largest volume but the least density, and that one which displaces the least water is the heaviest."

Abu-r-Raihan gives in tabular form the specific gravity of nineteen substances, nine of which are minerals and nine are stones. In the following table we annex the values assigned by modern authorities, showing the remarkable accuracy, in most instances, of the early determinations:—

TABLE OF SPECIFIC GRAVITIES.

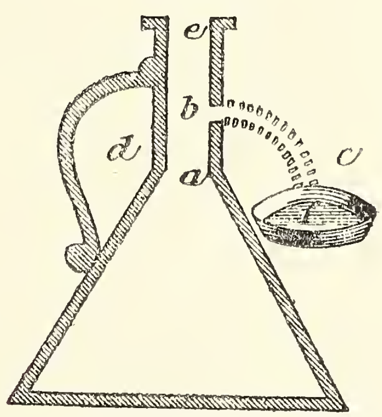
	Abu-r-Raihan.	Modern Authorities.
Gold	19·05	19·30
Mercury	13·58	13·568
Lead	11·33	11·346
Silver	10·35	10·52
Bronze	8·82	8·05 to 8·95

* Journal Asiatique. Serie v., vol. xi., p. 379.

	Abu-r-Raihan.	Modern Authorities.
Copper	8·70	8·78
Brass	8·57	8·58
Iron	7·74	7·79
Tin	7·31	7·29
Sapphire.	3·97	3·99
Oriental Ruby	3·58	3·90
Ruby	3·85	3·52
Emerald	2·75	2·73
Pearl	2·69	2·75
Lapis Lazuli	2·60	2·90
Cornelian	2·56	2·61
Amber (?)	2·53	1·08 (?)
Rock crystal	2·50	2·58

In the third lecture of the "Book of the Balance of Wisdom" al-Khazini describes a form of specific gravity flask which he calls the "conical instrument of Abu-r-Raihan" and to whom he apparently ascribes the invention. A mere inspection of the accompanying cut, a fac-simile of that in the original manuscript, together with the explanations (also from the original), suffices to show its nature and the method of using it. The author remarks that "the instrument is

Fig. 1.



a. Neck of the instrument. *b.* Perforation. *c.* Tube in the form of a water-pipe. *d.* Handle of the instrument. *e.* Mouth of the instrument. *f.* Place of the pan (of the balance).

very difficult to manage, since very often the water remains suspended in the lateral tube, dropping from it little by little into the scale of the balance." This passage shows that Abu-r-Raihan had noticed capillary attraction; it is also certain that he understood that the size of the neck of the instrument affected the delicacy of the determinations, for he says he would have it "made narrower than the little finger but for the difficulty of removing through a smaller tube the bodies immersed in the water."

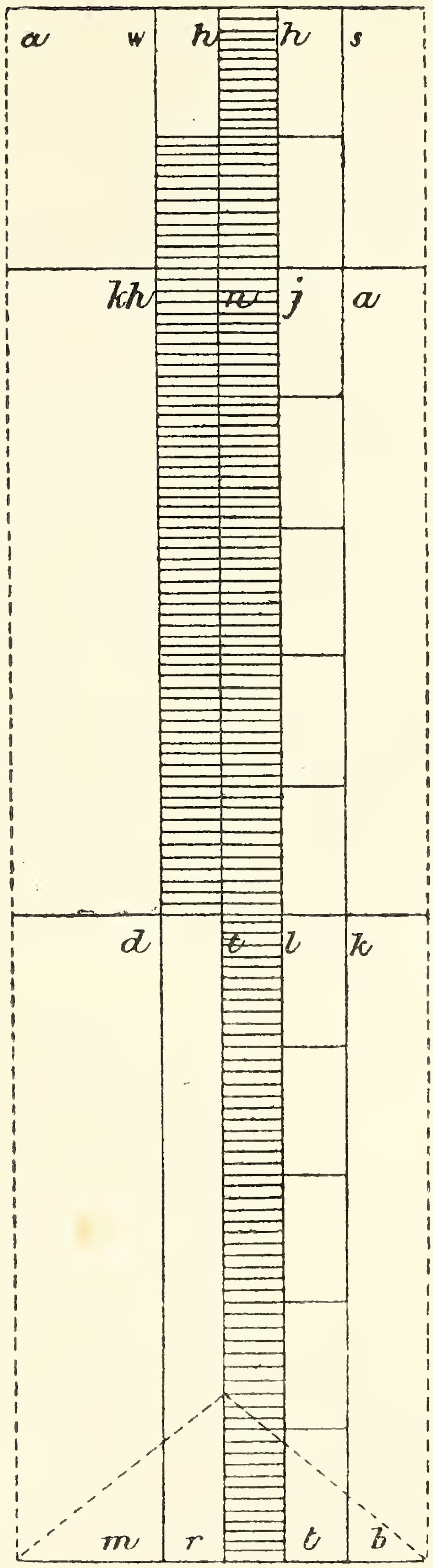
Al-Khazini's work is made up of eight lectures, each lecture includes several chapters and each chapter has several sections; to give the table of contents entire would undesirably lengthen this article, and we prefer to quote Al-Khazini's own summary of his treatise as contained in the sixth section of the introduction.

"I have divided the book into three parts:—1. General and fundamental topics, such as heaviness and lightness; centres of gravity; the proportion of the submergence of ships in water; diversity of the causes of weight; mechanism of the balance and the steelyard; mode of weighing with it in air and in liquids; the instrument for measuring liquids, in order to ascertain which is the lighter and which is the heavier of the two, without resort to counterpoises; knowledge of the relations between different metals and precious stones in respect to [given] volume; sayings of ancient and modern philosophers with regard to the water balance and their intimations on the subject. This part includes four lectures of the book in their order. II. Mechanism of the balance of wisdom; trial of it; fixing upon it of [the points indicating] the specific gravities of metals and precious stones; adoption of counterpoises suited to it; application of it to the verification of metals and distinguishing of one from another [in a compound], without melting or refining, in a manner applicable to all balances; recognition of precious stones and distinction of the genuine from their imitations or similitudes in colour. There are here added chapters on exchange and the mint, in connection with the mode of proceeding, in general, as to things saleable and legal tenders. This part embraces three lectures. III. Novelties and elegant contrivances in the way of balances such as: the balance for weighing dirhams and dinars without resort to counterpoises; the balance for levelling the earth to the plane of the horizon; the balance known as the 'even balance' which weighs from a grain to a thousand dirhams or dinars by means of three pomegranate counterpoises; and the hour-balance, which makes known the passing hours, whether of the night or of the day, and their fractions in minutes and seconds, and the exact correspondence therewith of the ascendant star and fractions of a degree. This part is in one lecture."

In the seventh chapter, which treats of the "Mechanism of the Instrument for measuring Liquids . . . and Application of it according to the philosopher Pappus the Greek," we find a description of a hydrometer.

"The length of this instrument, which is cylindrical in

Fig. 2.



shape, measures half a hand-cubit; and the breadth is equal to that of two fingers, or less. It is made of brass, is hollow, not solid, and the lighter particles of brass are care-

fully turned off by the lathe. It has two bases, at its two ends, resembling two light drum-skins, each fitted to the end, carefully, with the most exact workmanship; and on the inner plane of one of the two bases is a piece of tin, carefully fitted to that plane by the lathe, shaped like a funnel, the base of which is the drum-skin itself. The instrument being thus made, when put into liquid in a reservoir or vessel, it stands upon it in an erect position and does not incline any way."

The author then describes at length the manner of graduating the instrument, the decimal system being employed throughout. He remarks that the weight of the funnel-shaped piece of tin must be varied according to the density of the water assumed as a standard. Tables of the specific gravities corresponding to the marks on the instrument accompany the detailed account of its application.

The annexed figure of the hydrometer of Pappus (Fig. 2) does not give a very clear idea of the instrument, and is intended to show chiefly the manner of constructing and graduating it. The details given in the manuscript are so minute, however, that it is evident Pappus's instrument resembled closely that of Gay-Lussac. It was, however, provided with two scales, one with its numbers increasing upwards to indicate the volume submerged in liquids of different density; the other with its numbers increasing downwards, to show the specific gravities corresponding to those submerged volumes.

The above-mentioned Pappus was an eminent Greek geometer of Alexandria, who flourished about 380 or 400 A.D. Consequently he was a contemporary of Synesius of Cyrene (378—430 A.D.), in one of whose letters occurs what is ordinarily regarded as the first recorded mention of the hydrometer. It is certainly most interesting to find that al-Khazini's description of Pappus's instrument corresponds very closely with the statements of Synesius—a coincidence not observed by previous writers.

Synesius, "the good bishop of Ptolemais," writing to his instructress,* the fair Pagan philosopher and mathematician, the ill-fated Hypatia, and being desirous of trying the wines† he is using, says—

"My health is so delicate that I need a *hydroscope*, and I beg you to have one made for me of copper. It is a tube, cylindrical in shape, and of the form and size of a pipe; on

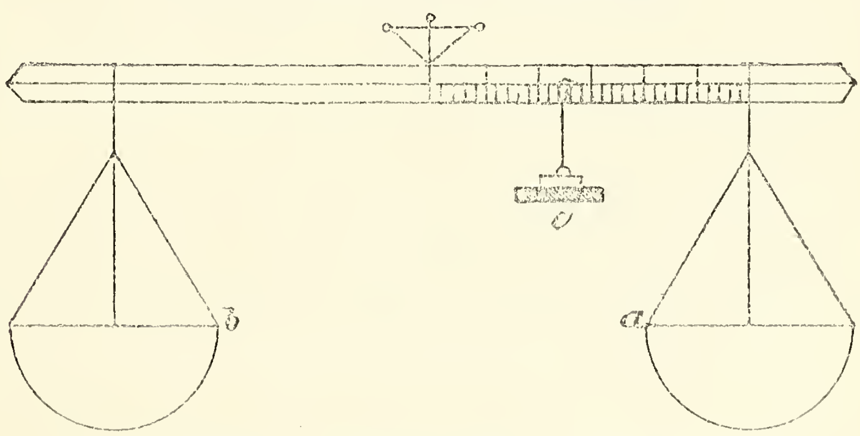
* Not in a letter of Hypatia to Synesius, as Hoefer has it in his Hist. Physique; Paris, 1872, 12mo.

† DRAPER, Hist. Int. Devel. Europe.

its length it bears a straight line crossed by small lines, by means of which we determine the weight of waters. One end is terminated by a cone, arranged in such a manner that the tube and the cone have the same base. This instrument is called *baryllion*. If you place it in water, point downwards, it stands erect, and the divisions that cross the vertical line can be easily counted, and by this means the density of water is determined."*

Hoefer, the French Historian of Chemistry, in relating this statement, remarks that none of the commentators of the Letters of Synesius were able to explain the nature of this instrument until the mathematician Fermat, in answer to Castelli's request, communicated his view, correctly apprehending the principles and uses of the instrument described. This was in 1628, and now we learn that the Arabian philosophers five centuries earlier were perfectly

FIG. 3.—BALANCE OF ARCHIMEDES.



a. Bowl for gold. *b.* Bowl for silver. *c.* Movable weight.

familiar with the identical instrument mentioned by Synesius.

Al-Khazini describes several forms of balances at great length, giving details of construction and employment. One of these balances he ascribes to Archimedes; and he professes to quote the particulars respecting it from Menelaus, without however giving the title of the latter's work.

Another balance described by our author is that of Muhammed Bin Zakariya of Rai; it differs from that of Archimedes by the introduction of the needle, called by the Arabs the "tongue," and by the substitution of a movable suspended bowl for the movable weight.

Finally, in the fifth lecture, he gives a minute description

* Synesius Opera, Epist. XV., Lutetia, 1612, 4to, p. 174.

of the Balance of Wisdom according to Abu-Hatim al-Muzaffar Bin Ismail, of Isfazar. "He begins by remarking that the balance being an instrument of precision, like astronomical instruments such as the astrolabe and the zijassafaih, its whole workmanship should be carefully attended to. He next describes the beam, the front piece, the two 'cheeks' between which the 'tongue' moves, and the tongue itself." He gives the length of the beam as 4 bazaar cubits (2 metres), and remarks that "length of the beam influences the sensibility of the instrument;" it is constructed of iron or bronze. The balance is provided with five bowls or pans, made of very thin plates of bronze, three of which have the form of hemispheres (see Fig. 4), one of which is spherical, and the remaining one, destined to be plunged into water, is finished with a conical bottom. Two of these bowls bore the name of the "aërial," and were permanently attached to the beam; another pan was movable on the right arm of the beam; and the bowl intended to be immersed in water was fastened underneath the aërial bowl of the left arm: this bowl bore the name of the "aquatic," and the spherical bowl was named the "winged."

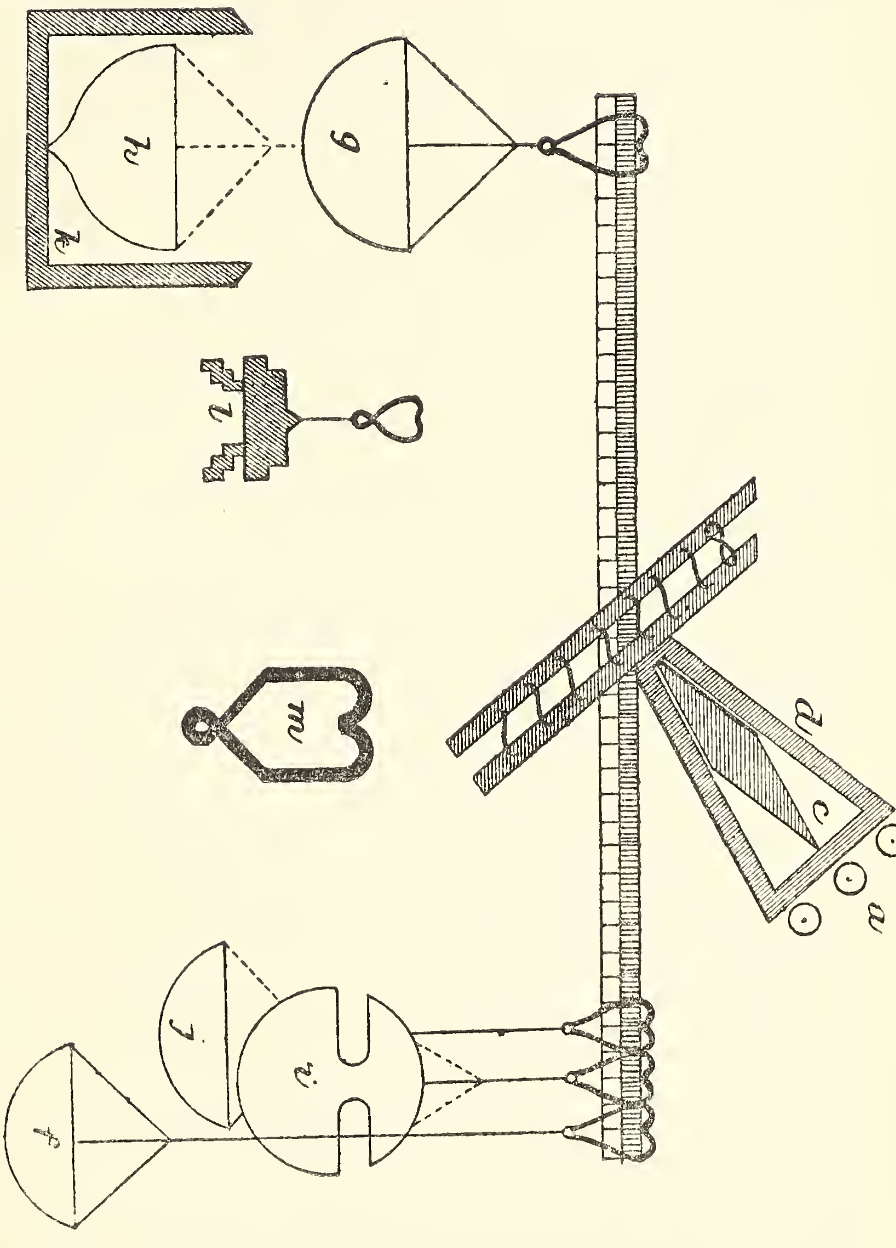
We cannot here enter upon a more detailed examination of this portion of al-Khazini's treatise; suffice it to say, he speaks of the mode of adjusting the balance and of its application to the examination of metals and of precious stones. Al-Khazini distinctly states that in taking the specific gravity of bodies he employed "a determined sort of water, similar in density to the water of the Jaihun of Khuwarazm," and further that "we made all our comparisons in one single corner of the earth, namely, in Jurjaniyah [a city] of Khuwarazm, ... and early in the autumnal season of the year." The "Jaihun" is the modern river Oxus, and "Khuwarazm" corresponds to the modern province of Khiva.

The editor, M. Khanikoff, calls special attention to the following passages, which he considers the most remarkable in the whole treatise:—

"When a heavy body, of whatever substance, is transferred from a rarer to a denser air, it becomes lighter in weight; from a denser to a rarer air it becomes heavier." —(Lect. I., Chap. v., § 1.)

"Air-weight does not apparently vary, although there is actual variation, owing to difference of atmospheres. As regards its water-weight, a body visibly changes according to the difference between waters of [different] regions, wells,

FIG. 4.—BALANCE OF WISDOM (*one-third the size of original drawing*).



a. Means of suspension.	*f.* Air-bowl.	*i.* Winged bowl.	*l.* Pomegranate counterpoise.
c. Tongue.	*g.* Second air-bowl.	*j.* Movable bowl.	*m.* Rings of suspension.
d. Two cheeks.	*h.* Water-bowl.	*k.* Basin.	

and reservoirs, in respect to rarity and density, together with the incidental difference due to the variety of seasons and uses. So then the water of some determined region and known city is selected, and we observe upon the water-weight of the body, noting exactly what it is, relatively to the weight of 100 mithkals; and we refer [all] operations to that [result as a standard], and keep it in mind against the time when we are called upon to perform them, if the Supreme God so wills. In winter one must operate with tepid, not very cold water, on account of the inspissation and opposition to gravity of the latter, in consequence of which the water-weight of the body [weighed in it] comes out less than it is found to be in summer. This is the reason why the water-bowl settles down when the water has just the right degree of coldness, and is in slow motion, while in case it is hot and moving quickly, or of a lower temperature, yet warmer than it should be, the bowl does not settle down as when the water is tepid. *The temperature of water is plainly indicated, both in winter and summer;* let these particulars therefore be kept in mind."—(Lect. V., Chap. vi., § 5.)

An examination of these extracts compels a belief that the Arabian philosophers of the twelfth century knew the air to have weight, though they never applied the means they had discovered of measuring it. The sentence in italics leads to the conjecture that they also had some means of determining the temperature of water; possibly a form of aërometer was the instrument employed, and they were thus enabled to recognise the fact that the density of water increases in proportion to its coldness.

Al-Khazini's work contains several tables of the specific gravities of substances determined either by the Balance of Wisdom or by the hydrometer of Pappus. In these tables are enumerated fifty substances, nine of which are metals, ten precious stones, thirteen materials of which models were made, and eighteen liquids. The smallness of the list is not surprising, for most of the substances contained in modern lists of specific gravities were entirely unknown to the Arabians; the exactness of the results obtained is marvellous, when we take into consideration the coarseness of their means of graduating instruments and the backward state of the mechanical arts at that period.

The first table comprises the specific gravities of seven metals and two alloys; the results, interpreted into our system, together with the values assigned by modern authori-

ties,* are found below. For sake of comparison, we also annex the figures obtained by Abu-r-Raihan, from whom it is believed al-Khazini quotes; the slight discrepancies are largely due to different methods of calculation adopted by Clément-Mullet and Khanikoff.

AL-KHAZINI'S FIRST TABLE OF SPECIFIC GRAVITIES.

Substances.	Al-Khazini.	Abu-r-Raihan.	Modern Authorities.
Gold [cast]	19·05	19·05	19·30
Mercury	13·56	13·58	13·59
Lead	11·32	11·33	11·34
Silver	10·30	10·35	10·52
Bronze	8·82	8·82	8·05 to 8·95
Copper	8·66	8·70	8·78
Brass	8·57	8·57	8·58
Iron (forged)	7·74	7·74	7·79
Tin	7·32	7·31	7·29

It is interesting to learn that the Arabian physicists fully appreciated the necessity of operating on pure materials, and the advantages of averaging the results of many determinations.

Thus al-Khazini says he purified gold by melting it five times, after which it melted with difficulty, solidified rapidly, and left hardly any trace upon the touchstone; and, after ten trials to obtain the weight of the volume of water displaced by different weights of the gold, he found, for a hundred mithkals of gold, weights varying from 5 mithkals 1 danik and 1 tassuj to 5 mithkals 2 daniks; as mean weight he adopts 5 mithkals 1 danik 2 tassujs, which by calculation yields the figures in the preceding table.

Likewise mercury was purified by passing it repeatedly through many folds of linen cloth. In writing of mercury, he remarks that it is not, properly speaking, a metal, but it is "the mother of the metals, as sulphur is their father." This view of the nature of mercury was prevalent among Arabian chemists, and is found in the writings of Geber (or Djafar), who lived four centuries earlier. Geber writes of mercury—"It is also (as some say) the matter of metals with sulphur,"† and he does not place it in the same class with metals which he defines as "extensible under the hammer," a property not possessed by mercury under ordinary conditions.

* Cf. Prof. F. W. CLARKE's Tables of Specific Gravities, &c., in Constants of Nature, Part I., Smithsonian Miscellaneous Collections, 1873, 8vo.

† GEBER, Sum of Perfection, Book I., Part III., Chap. 6.

In this conception of al-Khazini we find, moreover, the germs of the doctrine of the transmutation of metals, the basis of that alchemical pseudo-science which subsequently acquired such a wonderful influence over the human race. For if metals have mercury for a mother and sulphur for a father, they are not simple substances, and if compounds, they are capable of artificial preparation and mutual transformation. This is, however, not the only passage containing allusions to a belief in transmutation, though no mention occurs of any practical attempts to effect it; the following extract clearly refers to the compound nature of metals.

"When the common people hear from natural philosophers that gold is the most equal of bodies, and the one which has attained to perfection of maturity, at the goal of competition in respect to equilibrium, they firmly believe that it is something which has gradually come to that perfection by passing through the forms of all [other metallic] bodies, so that its gold nature was originally lead, afterwards became tin, then brass, then silver, and finally reached the perfection of gold."

Writing of the precious metals, al-Khazini discourses in a philosophical spirit on their universal appreciation, in the following language:—

"'Men prize these metals,' says Abu-r-Raihan, 'only because under the action of fire they admit of being made into conveniences for them, such as vessels more durable than others, instruments of agriculture, weapons of war, and other things which no one can dispense with who is set to possess himself of the good things of life and is desirous of the adornments of wealth.' But if besides the rarity of the occurrence of gold, its durability, and the little appearance of moisture upon it, whether moisture of water or humidity of the earth, or of its being cracked or calcined by any fire, and consumed together; with its ready yielding to the stamp, which prevents counterfeiters from passing off something else for it, and lastly the beauty of its aspect—if there is not [besides all these characteristics] some inexplicable peculiarity pertaining to gold, why is the little infant delighted with it, and why does he stretch himself out from his bed in order to seize upon it? And why is the young child lured thereby to cease from weeping, although he knows no value that it has, nor by it supplies any want? And why do all people in the world make it the ground of being at peace one with another, not drawing their swords to fight, though at the sacrifice of the powers of body and soul, of family connections, children, ground possessions,

and everything, with even a superfluity of renunciation for the sake of acquiring that; and yet are ever longing for the third stream* to stuff their bellies with the dust?"

This passage is a sample of the simplicity of much of the "Book of the Balance of Wisdom;" and, occurring in the midst of the purely scientific demonstrations and data, is peculiarly refreshing; the author's testimony to that inexplicable peculiarity of gold which renders it the special object of avarice, leads us to conjecture that had he lived in modern times he would have proved a warm champion of "hard money" doctrines.

The second table of specific gravities contains the determinations of various precious stones; it is not possible in every case to identify the stones, and hence some uncertainty obtains with regard to the values:—

Substances.	Specific Gravities.	
	Al-Khazini.	Modern Authorities.
Celestial hyacinth	3·96*	4·83
Red hyacinth	3·85†	3·99
[Ruby] of Badakhshan	3·58	—
Emerald	2·75	2·68 to 2·77
Lapis Lazuli	2·69	2·90
Fine pearl	2·60	2·68
Cornelian	2·56	2·62
Coral	2·56	2·69
Onyx and crystal	2·50	2·63‡ to 2·88§
Pharaoh's glass	2·49	2·45‖ to 3·44**

Al-Khazini gives detailed accounts of these precious stones, of which we quote a few brief extracts. He says:—"Emerald and chrysolite are interchangeable names, whether applied to one and the same thing, or to two things of which one has no real existence," a passage which shows that mineralogical terminology was afflicted with superfluous synonyms at an early day. Of the cornelian he says:—"Men have long tired of the cornelian, so that it has ceased to be used as a stone for seal-rings; even for the hands of common people, to say nothing of the great." Of the "fine pearl," he writes:—"The pearl is not a stone at all, but only the bone of an animal, and not homogeneous in its parts."

* Alluding, say the editors, to the traditional saying of the Arabians: "If the son of Adam were to possess two flowing rivers of gold and silver, doubtless he would desire a third."

* Oriental sapphire.
† Oriental ruby.
‡ Onyx 2·63 to 2·81.
§ Rock crystal 2·68 to 2·88.
‖ English mirror glass.
** English flint glass.

"Coral," he writes, "is a plant, though petrified like the Jew's stone and the sea-crab." He is aware that "glass is not the product of a mine, but, on the contrary, kindred to stones, or sand, or alkali," and he states that he has included it in the above list, "because it resembles crystal."

The third table comprises "the materials of models and patterns formed by goldsmiths, and woods of well-known trees." Interpreting the Arabic weights as before, we have the following table:—

Substances.	Specific Gravities.	
	Al-Khazini.	Modern Authorities.
Clay of Siminjan	1·99	clay 1·068 to 2·63
Pure salt	2·19	2·068 to 2·17
Saline earth	1·11	—
Sandarach	0·71	1·05 to 1·09
Amber	0·85	1·06 to 1·08
Enamel	3·93	—
Pitch	1·04	1·07
Wax	0·95	0·96
Ivory	1·64	1·82
Ebony	1·13	1·18
Pearl shell	2·48	2·64
Bakkam wood	0·94	1·03
Willow wood	0·40	0·58

The last table embraces a number of liquids.

Substances.	Specific Gravities.	
	Al-Khazini.	Modern Authorities.
Sweet water	1·000	1·000
Hot water	0·958	0·959
Ice	0·965	0·916 to 0·926
Sea water	1·041	1·028 to 1·040
Water of Indian melon	1·016	—
Salt water [saturated solution]	1·144	1·205
Water of cucumber	1·017	—
Water of common melon	1·030	—
Wine vinegar	1·027	—
Wine	1·022	1·013
Oil of sesame	0·915	0·992 to 1·038
Olive oil	0·920	0·917 to 0·919
Cow's milk	1·110	1·029 to 1·040
Hen's egg	1·035	1·090
Honey	1·406	1·450
Blood of men in good health	1·033	1·053
Warm human urine	1·018	1·011
Cold human urine	1·025	

The temperatures at which the determinations of the "hot water" and "sweet water" were made are not known; the difference of density observed—viz., 0·04166—approximates that between water at 3·9° C. and 100° C., which according to modern physicists is equal to 0·04044.

The high specific gravity of cow's milk is noteworthy, and may have led al-Khazini and others into lactometrical controversy.

Besides these contributions to the knowledge of specific gravities, al-Khazini devotes some attention to certain subjects not closely connected with the main theme. In the third lecture he attempts to calculate the quantity of gold which would compose a sphere equal to the globe of the earth, and arrives at a number of mithkals which requires for expression 29 digits.

In the same lecture he takes up the problem of the chess-board, of which he supposes the squares to be filled with dirhams, each square containing twice the number in the preceding; he finds the total number of dirhams to be 18,446,744,073,709,551,615. He then applies himself to ascertaining the dimensions of the treasury in which the treasure should be deposited, and finally quotes the lines of an Arabian poet which fix the time in which one might spend this sum at 200,000,000,000,000,000 years.

In the last lecture he describes the methods of applying the balance to levelling and to the measuring of time. Of this portion M. Khanikoff gives the following concise exposition:—

"The balance level consists of a long lever, to the two ends of which were attached two fine silken cords, turning on an axis fixed to a point a little above its centre of gravity, and suspended between two sight-pieces of wood, graduated. At the moment when the lever became horizontal, the cords were drawn in a horizontal direction, without deranging its equilibrium, and the divisions of the scales of the sight-pieces corresponding to the points where the cords touched them were noted. For levelling plane surfaces, use was made of a pyramid with an equilateral triangular base, and hollow and open to the light, from the summit of which hung a thread ending with a heavy point. The base of the pyramid thus arranged was applied to the plane which was to be levelled, and carried over this plane in all directions. Wherever the plane ceased to be horizontal the joint deviated from the centre of the base.

"The balance-clock consisted of a long lever suspended similarly to the balance-level. To one of its arms was at-

tached a reservoir of water, which, by means of a small hole perforated on the bottom of it, emptied itself in twenty-four hours. This reservoir, being filled with water, was poised by weights attached to the other arm of the lever, and, in proportion as the water flowed from it, the arm bearing it was lifted, the weights on the other arm slid down, and by their distance from the centre of suspension indicated the time which had elapsed."

Further analysis of the contents of this extraordinary work is incompatible with our reader's patience, and yet many points of interest demand at least a passing notice: these may, however, be embodied in a summary of the principal propositions contained in this treatise, and the recapitulation may serve to justify in some measure this contribution to the early history of chemical physics:—

1. The "Book of the Balance of Wisdom" shows the Arabian philosophers of the twelfth century to have entertained advanced views regarding attraction. They recognised gravity as a force, and attributed to it a direction towards the centre of the earth; they also knew that it diminishes with the distance, but they erroneously supposed this diminution to be in the direct ratio of the distance and not as its square.
2. They were acquainted with the connection between the weight of the atmosphere and its increasing density, since mention is made of the loss of weight of a body weighed in a denser atmosphere.
3. They understood the theory of centre of gravity, and applied it to the investigation and construction of balance and steelyards.
4. They made frequent use of the hydrometer, which they inherited from antiquity, and possibly they employed this instrument as a thermometer for distinguishing by variations of density the different temperatures of liquids.
5. They observed the action of capillary attraction.
6. They compiled full and accurate tables of the specific gravities of most of the solids and liquids with which they were acquainted.
7. Their system of philosophy was founded on experiment and observation.

In conclusion, we quote the following appropriate remarks from M. Khanikoff's introduction:—

"The history of the sciences presents to us an incontestable fact of deep significance—the re-discovery in modern

times of truths laboriously established of old; and this fact is of itself enough to indicate the necessity of searching carefully in the scientific heritage of the past after all that it may be able to furnish us for the increase of our actual knowledge; for a double discovery, necessarily requiring a double effort of human intellect, is an evident waste of that creative force which causes the advance of humanity in the glorious path of civilisation."

VI. THE PHILOSOPHY OF THE RADIOMETER AND ITS COSMICAL REVELATIONS.

By W. Mattieu Williams, F.R.A.S., F.C.S.

So much speculation, and not a little extravagant speculation, has been devoted to the dynamics of the radiometer, that I feel some compunction in adding another stone to the heap, my only apology and justification for so doing being that I propose to regard the subject from a very unsophisticated point of view, and with somewhat heretical directness of vision,—*i.e.*, quite irrespective of atoms, molecules, or ether, or any other specific preconceptions concerning the essential kinetics of radiant forces beyond that of regarding such forces as affections or conditions of matter which are transmitted radially in constant quantity, and therefore obey the necessary law of radial diffusion, or inverse squares.

The primary difficulty which appears to have generally been suggested by the movements of the radiometer, is the case which it seems to present of mechanical action without any visible basis of corresponding reaction; a visible tangible object pushed forward, without any visible pushing agent or resisting fulcrum against which the moving body reacts.

This difficulty has been met by the invocation of obedient and vivacious molecules of residual atmospheric matter, which have been called upon to bound and rebound between the vanes and the inner surfaces of the glass envelope of the instrument.

How is it that the advocates of these activities have not sought to verify their speculations by modifying the shape and dimensions of the exhausted glass bulb or receiver? If the motion of the radiometer is due to such excursions and collisions, the length of excursion and the angles of

collision must modify its motions; and such modification under given conditions would form a fine subject for the exercise of the ingenuity of molecular mathematicians. If their hypothetical data are sound, they should be able to predict the relative velocities or torsion force of a series of radiometers of similar construction in all other respects, but with variable shapes and diameters of enclosing vessels.

If we divest our minds of all visions of hypothetical atoms, molecules, ethers, &c., and simply look at the facts of radiation with the same humility of intellect as we usually regard gravitation, this primary difficulty of the radiometer at once vanishes. The force of gravitation is a radiant force acting somehow between, or upon, or by distant bodies; and these bodies, however far apart, act and react upon each other with mutual forces, precisely equal and exactly contrary. We conceive the sun pulling the earth in a certain direction, and receiving from the earth an equal pull in a precisely contrary direction, and we have hitherto demanded no ethereal or molecular link for the transmission of these mutually attractive forces. Why, then, should we not regard radiant repulsive energy in the same simple manner?

If we do this there is no difficulty in finding the ultimate reaction fulcrum of the radiometer vanes. It is simply the radiating body, the match, the candle, the lamp, the sun, or whatever else may be the source of the impelling radiations. According to this view, the radiant source must be repelled with precisely the same energy as the arms or pendulum of the radiometer; and it would move backward or in opposite direction if equally free to move. If, by any means, we cause the glass envelope of the radiometer to become the radiant source, it should be repelled, and may even rotate in opposite direction to the vanes or *vice versa*. This has been done with floating radiometers.

Viewed thus as simple matter of fact, irrespective of any preconceived kinetics of intervening media, the net result of Mr. Crookes's researches become nothing less than the discovery of a new law of nature of great magnitude and the broadest possible generality, viz., that the sun and all other radiant bodies, *i.e.*, all the materials of the universe, exert a mechanical repulsive force, in addition to the calorific, luminous, actinic, and electrical forces with which they have hitherto been credited. He has shown that this force is refrangible and dispersible, that it is outspread with the spectrum, but is most concentrated, or active, in the region of the ultra-red rays, and progressively feeblest in the violet;

or otherwise stated—it exists in closer companionship with heat than with light, and closer with light than with actinism.

According to the doctrine of exchanges, which has now passed from the domain of theory to that of demonstrated law, all bodies, whatever be their temperature, are perpetually radiating heat-force, the amount of which varies, *cæteris paribus*, with their temperature. If we now add to this generalisation that all bodies are similarly radiating mechanical force and suffering corresponding mechanical reaction, the difficulties vanish. What must follow in the case of a freely suspended body unequally heated on opposite sides?

It must be repelled in a direction perpendicular to the surface of its hottest side. If two rockets were affixed to opposite sides of a pendent body, and were to exert unequal ejective forces, the reaction of the stronger rocket would repel the body in the opposite direction to its preponderating ejection. This represents the case of the radiometer vane with one side blackened and the other side bright. When exposed to luminous rays, the black side becomes warmer than the bright side, by its active absorption and conversion of light into heat, and thus the blackened face recedes.

We may regard it thus as reacting by its own radiations, or otherwise as acted upon by the more powerful radiant whose rays are differentially received by the black and bright sides. These different modes of regarding the action are perfectly consistent with each other, and analogous to the the two different modes of regarding gravitation, when we describe the sun as attracting the earth, or, otherwise, the earth as gravitating to the sun. Strictly speaking, neither of these descriptions is correct, as the gravitation is mutual, and the total quantity exerted betwen the sun and the earth is equal to the sum of their energies, but it is sometimes convenient to regard the action from a solar standpoint, and at others from a terrestrial. So with the radiometer and the strictly mutual repulsions between it and the predominating radiant.

It appears to me that this unsophisticated conception of radiant mechanical repulsive force, and its necessary mechanical reaction on the radiant body, meets all the facts at present revealed by the experiments of Mr. Crookes and others.

The attraction which occurs when the disc of the radiometer is surrounded with a considerable quantity of atmospheric matter is probably due to inequality of atmo-

spheric pressure. The absorbing face of the disc becomes heated above the temperature of the opposite face, the film of air in contact with the warmer face rises, leaving a relatively vacuous space in front. This produces a rush of air from back to front which carries the radiometer vane with it. When the exhaustion of the radiometer is carried so far that the residual air is only just sufficiently dense to neutralise the direct repulsion of radiation, the neutral point is reached. When exhaustion is carried beyond this, repulsion predominates.

Taking Mr. Crookes's estimate of the mechanical energy of solar radiation at 32 grains per square foot, 2 cwts. per acre, 57 tons per square mile, &c., and accepting these as they are offered, *i.e.*, merely as provisional and approximate estimates, we are led to a cosmical inference of the highest importance, one that must materially modify our interpretations of some of the grandest phenomena of the universe. Although the estimated sunlight pressure upon the earth, the three thousand millions of tons, is too small a fraction of the earth's total weight to effect an easily measurable increase of the length of our year, the case is quite otherwise with the asteroids and the zones of meteoric matter revolving around the sun.

The mechanical repulsion of radiation is a superficial action, and must, therefore, vary with the amount of surface exposed, while that of gravitation varies with the mass. Thus the ratio of radiant repulsion to the attraction of gravitation goes on increasing with the subdivision of masses, and becomes an important fraction in the case of the smaller bodies of the solar system. A zone of meteorites travelling around the sun would be broken up, sifted, and sorted, into different orbits according to their diameters, if this superficial repulsion operated against gravitation without any compensating agency. Gravitation would be opposed in various degrees, neutralised, and even reversed, in the case of cosmic dust. Comets presenting so large a surface in proportion to their mass, would either be driven away altogether or forced to move in orbits utterly disobedient to present calculations. This would occur if the interplanetary spaces were as nearly vacuous as the torsion instrument with which Mr. Crookes made his measurements.

Regarding the properties of our atmosphere only in the light of experimental data, irrespective of imaginary molecules, and their supposed gyrations or oscillations, we see at once that an inter-planetary or inter-stellar vacuum must act like a Sprengel pump upon our atmosphere, upon

the atmosphere of other planets, and upon those of the sun and the stars, and would continue such action until an equilibrium between the repulsive energy of the gas and the gravitation of the solid orbs had been established. Atmospheric matter would thus be universally diffused, with special accumulations around solid orbs, varying in quantity with their respective gravitating energy. Such a universal atmosphere would accelerate orbital motion, and this acceleration would vary with the surface of bodies. Its action being thus exactly opposed to that of radiant repulsion, it must, at a certain density, exactly neutralise it. That it does this is evident from the obedience of all the elements of the solar system to the calculated action of gravitation, and thus Mr. Crookes's researches not only confirm the idea of universal atmospheric diffusion, but they afford a means by which we may ultimately measure the actual density of the universal atmosphere. If, as I have endeavoured to show, in my essay on "The Fuel of the Sun," the initial radiant energy of every star depends upon its mass, and its consequent condensation of atmospheric matter, the density of inter-planetary atmosphere sufficient to neutralise the radiant mechanical energy of our sun may be the same as is demanded to perform the same function for all the stars of the universe, and all their attendant worlds, comets, and meteors.

In order to prevent misunderstanding of the above, I must add that I have therein studiously assumed a negative position, in reference to all hypothetical conceptions of the nature of heat, light, &c., and their modes of transmission, simply because I feel satisfied that the subject has hitherto been obscured and complicated by overstrained efforts to fit the phenomena to the excessively definite hypotheses of modern molecular mathematicians. The atoms invented by Dalton for the purpose of explaining the demonstrated laws of chemical combination, performed this function admirably and had great educational value, so long as their purely imaginary origin was kept in view, but when such atoms are treated as facts, and physical dogmas are based upon the assumption of their actual existence, they become dangerous physical superstitions. Regarding matter as continuous, *i.e.*, supposing it be simply as it appears to be—and co-extensive with the universe, in accordance with the experimental evidences of the unlimited expansibility of gaseous matter; we need only assume that our sensations, and the other phenomena of heat, light, &c., are produced by active conditions of such matter analogous to those which are

proved to produce our sensations, and the other phenomena of sound. On this basis there is no difficulty in conceiving the rationale of the reaction which produces the repulsion of the radiometer. I may even go further, and affirm that with this basis to our philosophy, it is impossible to rationally conceive radiation without mechanical reaction. If heat be motion, and actual motion of actual matter, mechanical force must be exerted to produce it, and a body which is warmer on one side than the other, *i.e.*, which is exerting more outward motion-producing force on one side than on the other, must be subject to proportionally unequal reaction, and therefore, if free to move, must retreat in a direction contrary to that of its greater activity. Regarded thus the residual air of the radiometer does act, not by collisions of particles between the vane and inside of the glass vessel, but by the direct reaction of the radiant energy which would operate irrespective of vessels, *i.e.*, upon naked radiometer vanes if carried half way to the moon, or otherwise freed from excess of atmospheric embarrassment.

The recent experiments of Mr. Crookes, showing retardation of the radiometer with extreme exhaustion, seem to indicate that heat-rays, like the electric discharge, demand a certain amount of atmospheric matter as their carrier.

I cannot conclude these hasty and imperfect notes, written merely with suggestive intent, without quoting a passage from the preface to the "Correlation of Physical Forces," which, though written so long ago, appears to me worthy of the profoundest present consideration.

"It appears to me that heat and light may be considered as affections; or, according to the undulatory theory, vibrations of matter itself, and not of a distinct ethereal fluid permeating it: these vibrations would be propagated just as sound is propagated by vibrations of wood or as waves by water. To my mind all the consequences of the undulatory theory flow as easily from this as from the hypothesis of a specific ether; to suppose which, namely, to suppose a fluid *sui generis* and of extreme tenuity penetrating solid bodies, we must assume, first, the existence of the fluid itself; secondly, that bodies are without exception porous; thirdly, that these pores communicate; fourthly, that matter is limited in expansibility. None of these difficulties apply to the modification of this theory which I venture to propose; and no other difficulty applies to it which does not equally apply to the received hypothesis."

NOTICES OF BOOKS.

The Origin of the Stars, and the Causes of their Motions and their Light. By JACOB ENNIS. London: Trübner and Co. New York: D. Appleton and Co.

THIS book offers us a solution of some most difficult questions. In his prefatory address the author directs the attention of astronomers to the following discoveries contained in his book:—He pronounces that gravitation is not merely the force which now holds the stars in their orbits, but which first set them in motion. He adds "a catalogue of some of the recent discoveries which have sprung from our knowledge of the projectile or centrifugal force." This catalogue we venture to quote:—

"1. The demonstration that the nebular theory of the origin of the universe is true: not the nebular theory of Laplace, but a theory far more comprehensive, and now first unfolded in this volume.

"2. The discovery that both the centripetal and centrifugal forces in astronomy, although antagonistic, are in all cases due to the same origin, the force of gravity.

"3. The discovery that gravity was the force which originally gave all their peculiar arrangements to all stellar systems, whether they be sidereal, solar, or planetary. It determined not only the velocities of the celestial bodies, but the direction of their movements, the size and positions of their orbits, and even their separate existences.

"4. The reason why the four outer planets have all the satellites but one, and why of the four inner planets the Earth alone has a satellite.

"5. Why there is no planet interior to Mercury.

"6. Why the distance of the innermost planet, Mercury, is 35,000,000 miles from the sun, whilst the distance of the innermost satellites is only about a quarter of a million miles from their primary planets.

"7. Why the interplanetary spaces are so large, from 32,000,000 miles to 1,000,000,000 miles, while the intersatellitic spaces are only from 134,000 to 1,300,000 miles.

"8. Why Saturn has so many more satellites and rings than Jupiter.

"9. Why the interplanetary spaces become less as we approach the sun, and why the last interplanetary space is an exception.

"10. Why the same facts and the same exceptions occur in the systems of Saturn and Uranus.

" 11. Why the orbits of the planets and satellites have become elliptical.

" 12. The reason why the orbital planes of the planets are not in the equatorial planes of the sun ; why the orbital planes of the satellites are not in the equatorial planes of the planets ; why the axes of the planets and satellites are not perpendicular to their orbital planes ; and why the rotation of Uranus, and possibly that of Neptune is retrograde.

" 13. Why the inner rings of Saturn remain unbroken.

" 14. The explanation of the ring of the asteroids, probably many hundred small plants in a belt by themselves.

" 15. The explanation of the origin of the milky way.

" 16. Why all planetary and solar systems move rapidly through space, and why all sidereal systems are stationary."

The author also seeks to demonstrate the cause of stellar light and heat to be chemical action. From his remarks on this subject we take the following extracts :—" The ignition of meteors proves that our air extends considerably higher than 205 miles. Even before reaching that point the amount of latent heat in the upper regions of the air is something wonderful. When this very rare air is compressed in front of a swiftly-shooting meteorite, the repulsion is overcome and the heat becomes sensible. The air is then heated to ignition, and often vitrifies the surface of the meteorite. In my paper on 'Meteors,' published in the 'Proceedings of the American Association for the Advancement of Science,' 1871, I have given examples of meteors which have been known to travel horizontally a thousand miles and more through the upper strata of the atmosphere, and continue vividly bright through the whole distance. I gave also examples of other meteorites which came down nearly vertically, and, although in broad daylight they glowed when high up as brightly as the sun, nevertheless their light was invariably extinguished before reaching the earth. Had the meteors themselves been incandescent, their brightness would have continued another twinkling of the eye until they struck the earth. Meteors are bright in the upper regions of the air, because of the abundance of latent heat up there. Their brightness goes out in the lower and denser strata of the air, because there is much less latent heat, although the *friction* is so much greater." He then contends that it is impossible for us to say how much chemical force can be stored up in the unknown chemical elements in the sun, or in their compounds. He declares that " the very greatest of all the lessons taught by the spectroscope has hitherto been overlooked by scientific men ; this lesson is the wide difference between the earth and the sun as regards their simple elements." Spectroscopists also let slip another truth, which is that " there are thousands of fixed lines in the solar spectrum which are perfect strangers to us."

In short, the author contends that the sun contains many elements not present in our earth, and that, such being the case, all computations on the length of time during which a mass of matter like the sun could continue to burn are necessarily illusive.

His remarks on the assumed identity of matter throughout the universe are not without interest to chemists. It is plain that he does not regard our present chemical elements as essentially and primordially distinct bodies, but as mere modifications or compounds of materials as yet unknown.

We have thus laid before the reader some specimens of this extraordinary work. Space will not permit us to give the evidence adduced by the author in support of his positions, especially as it is of a cumulative nature. We by no means see our way to a general acceptance of his conclusions, and we entertain strong doubts concerning some of his fundamental facts. To the following passage we take decided exception :—" It (the objection to the chemical theory of stellar light and heat) assumes, thirdly, that no new chemical elements can be formed in the sun,—that the materials of the sun cannot be decomposed or metamorphosed so as to be burnt over again. Here we can decompose water, and burn the oxygen and hydrogen a hundred times ; but we lose as much force or heat in the decomposing as we gain in the burning. But in the sun the case may be very different." We can as readily conceive that in the sun two and three are equal to six, as that a compound there can be decomposed without a consumption of force equal to that which was liberated during its composition. We have carefully read the section in which the author undertakes to prove that new combustibles may be prepared in the sun from matter which has already been burned ; but we cannot say that he has made out his case.

But for all this we are far from condemning the work before us. The author seeks not to overturn, but to extend and supplement the work of his predecessors in astronomy and physics, and even where he—in our opinion—has failed his labours cannot fail to prove useful.

The Moon, and the Condition and Configurations of its Surface. By EDWARD NEISON, F.R.A.S. London: Longmans and Co. 1876.

A BOOK on the moon, dealing specially with selenographical relations, is needed. In fact, lunar literature is incomplete for want of such a work. Nasmyth's interesting treatise on the moon, besides being too costly for the observatory, deals rather with theoretical than practical selenography ; and Proctor's treatise is general, not selenographical. There was room, therefore, for a treatise devoted specially to this department of lunar research. Unfortunately Mr. Neison has not been content to

produce such a treatise. He has gone over ground already well-trodden, and while his book has thus been so enlarged as to be as costly as Mr. Nasmyth's, it has been deprived of the special qualities which would have made it valuable to selenographers. It is, in fact, a general treatise on the moon.

The first chapter deals with the motions, figure, and dimensions of the moon; the second with her physical condition; the third with the various lunar formations; the fourth with what the author calls "lunar history," meaning thereby the history of lunar research; the fifth with the variations of the moon's surface; the sixth introduces the description of illustrative maps, to which the seventh, and many following chapters relate in detail; and the last chapter presents a number of formulæ, selenographical and otherwise. The chapter on the motions, figure, and dimensions of the moon is not satisfactory. For the most part, Mr. Neison quotes the descriptions given by French and German mathematicians. In some cases, by the way, he does not translate these descriptions correctly, as, for instance, at page 6, where the word "remplacer," used in the sense of "taking the place of," is represented by the English word "replace," which has quite a different, and, in this case, a quite incorrect sense.

In describing the motion of the lunar perigee, Mr. Neison writes, "The direction of the semi-axis major is not constant, but undergoes a slow revolution, the period of which is 8·8505 years, and in the same direction as the motion of the moon in its orbit, but with variable velocity, the lunar perigee being occasionally before and then behind its mean place." This description is incorrect. The motion of the perigee is not always in the same direction as the motion of the moon in her orbit and variable only in velocity. It varies in direction also, being sometimes progressive and sometimes retrogressive; advancing, on the whole, because the progressions are greater than the retrogressions. The reverse is the case with the motion of the nodes, which alternately retreat and advance, but on the whole retreat; whereas Mr. Neison describes this motion also as simply varying in velocity. The second chapter, on the moon's physical condition, is more original, but, apart from errors respecting physical laws, there is one remarkable error relating to a subject which Mr. Neison claims to have studied with special attention—the optical laws, namely, in which the refractive action of an atmosphere depends. "It may be remarked," he says, "that the rays, after traversing any atmosphere to the moon, would not be convergent as in a lens, but, owing to the refraction diminishing rapidly with the distance from the surface would be truly divergent; for the rays refracted by each spherical shell of atmosphere, as it were, of indefinably small thickness, would reach a different focus." According to this novel way of

dealing with the matter, the rays of a direct pencil refracted through a lens would be regarded as convergent or divergent, according as the aberration was towards or from the second surface of the lens, which we submit to be an entirely new view of convergency and divergency. Having, by reasoning satisfactory to himself, demonstrated the existence of a lunar atmosphere having probably about 1 - 300th part of the surface density of our atmosphere, Mr. Neison states that such an atmosphere is "not incapable of exerting as powerful influences on the surface as the earth's," and this because "its mass in proportion to that of the planet is only a little less than a fourth of that of the earth's, and with regard to even a single square mile in area of the surface, must be estimated by millions of tons." But in what way the largeness of the total mass of the atmosphere, or the pressure on a square mile of lunar surface, is to compensate for the fact that the surface density and pressure are certainly very minute, he in no way attempts to show. If the atmospheric pressure could be reduced anywhere on the earth till the mercurial barometer stood at one-tenth of an inch, the amount of air somewhere else, or the fact that even this reduced pressure was equivalent to some 900 millions of tons on the square mile, would not in the slightest degree tend to make that attenuated air equal in influence to air 300 times as dense.

The chapter relating to the history of lunar research is fairly accurate. Towards the close, however, Mr. Neison refers to the work done under the auspices of the British Association without mentioning the fact, that this work gave so little promise of leading to anything of value that the British Association withdrew its support. It is the general opinion of astronomers that the outlay of the Association was entirely thrown away.

The chapter on variations of the moon's surface contains little that has not been said elsewhere; but there is one passage relating to the supposed changes of the floor of Plato which is certainly incorrect. After describing these changes, Mr. Neison says that they are not "due to the effects of contrast, remaining unaffected when these are eliminated." The exact reverse is the case.

The next section, which indeed forms the bulk of the work, is occupied by descriptions of twenty-two maps. These remind us of Mädler's large map reduced in surface more than one half, cut up into twenty-two parts, and modified by omissions and additions, the law of which we fail to recognise. Some details are added which only the possessors of powerful telescopes could hope to examine with advantage, others are omitted which can be easily seen with a three inch object-glass. But the great mistake has been the division of the chart into many parts and the introduction of 3-into a cumbrous volume. The selenographer does not want a book of maps, still less a large volume in which such maps are accompanied by a quantity (in this case

nearly 600 pp.) of letterpress, but a chart which he can hang in his observatory.

With respect to the chapter headed Selenographical Formulæ, the remarks we have made on the first chapter seem entirely applicable, with this additional one, that a large proportion of the formulæ are not selenographical at all, and could have no value whatever, except to astronomers specially engaged in studying the lunar motions, who already possess these formulæ in books belonging to their subject.

Mr. Neison's book, necessarily large from the multitude of matters which he has undertaken to deal with in it, is made larger by repetitions. For instance, on page 133 a passage is repeated which occurs on page 132.

There is one point not peculiar to this book, though illustrated by it, which we should hardly deem important enough to notice were it not that it has attracted the attention of Continental astronomers, and been made the subject of specially unfavourable comment in America (in an article in the "Nation" for November, 1873, attributed, correctly we believe, to Prof. Newcomb, of the Washington Observatory). We refer to the nomenclature. When Beer and Mädler raised the number of names of lunar craters and other features to 427, one-third of which they added themselves, it was felt that the thing had been a good deal overdone, though the important contributions made by Beer and Mädler to lunar research prevented astronomers from expressing that opinion very strongly. But since Beer and Mädler's chart was published a number of new names have quite unnecessarily been added by Mr. Birt, and others, in such a way as to make the whole system of lunar nomenclature ridiculous. It ought to be understood that only such selenographers as Schröter, Lohrmann, Beer and Mädler, and Schmidt,—men who have constructed new and valuable lunar charts, not mere drawings of small lunar regions,—should venture to add new names to lunar maps, even if new names were wanted. As it is, one lunar student adds a set of names which he communicates to another (whose name is included); this one approves of them and adds other names, including that of his friendly correspondent. Mr. Birt sends his new list of names to Mr. Webb, who adds them to his already too lengthy list. Then Mr. Webb's name appears in a new list by Mr. Birt, and so on *usque ad nauseam*. We may be certain that neither American nor Continental astronomers, nor any English astronomers of repute, will adopt these new names,—at least until they have some selenographical contributions of far more importance from Mr. Birt than mere promises and a portfolio of lunar scraps.

The Argonaut. Edited by G. Gladstone, F.R.G.S., F.C.S. London: Hodder and Stoughton.

The "Argonaut" is a periodical differing from others of its class by the relatively large share of space it devotes to Science. Still we seem, in turning over its pages, to breathe a strange atmosphere. Much of the volume, too, lies fairly outside our sphere of criticism. Thus we find a series of papers on the "Use of the Supernatural in Art." We doubt not the ability here displayed, but in how far the author's views are sound we cannot venture to pronounce.

In a paper on the "Birth of Alchemy," by Prof. Gladstone, F.R.S., we find this strange pursuit traced much further eastwards than Egypt or Arabia, namely, to China, where it flourished from the sixth century before Christ. A work on the preparation of the Elixir, entitled the "Uniting Bond,"* was written by Wei-peh-yang during the Eastern Han Dynasty, some time from A.D. 25 to 221. An epitome of this work has been published by the Rev. Dr. Edkins in the "Miscellany or Companion to the Shanghai Almanack for 1857." We extract the following passage, which shows a strong resemblance to the alchemical writings of Western Asia and of Europe:—"The cauldron is round, like the full moon, and the stove beneath is shaped like the half moon. The lead ore is symbolised by the white tiger, which, like metal among the elements, belongs to the West. Mercury resembles the sun, and forms itself into sparkling globes. It is symbolised by the blue dragon, belonging to the East, and it is assigned to the element wood. Gold is imperishable. Fire does not injure its lustre. Like the sun and the moon, it is unaffected by time. Therefore the elixir is called the golden elixir. Life can be lengthened by eating the herb called *hu-ma*; how much more by taking the elixir which is the essence of gold, the most imperishable of all things. . . . Lead-ore and mercury are the basis of the process by which the elixir is prepared. They are the hinge on which the principles of light and darkness revolve."

The Elixir was not, however, as we may judge from its materials, found very successful in practice. Indeed two Emperors of the Tang Dynasty are said to have died from its effects—a result which very naturally ruined its *prestige*. The whole story has a double interest: not merely does it throw a new light on the origin of one of the strangest phases of the chemistry of the past, but it proves that China was not always the swamp of stagnant learning to which she has been reduced by her system of competitive examinations. Would that we might take the lesson to heart! Surely everyone can see that the man best able to "get up" a subject on short notice is not necessarily the man most capable of original thought!

* Have we here an anticipation of the notions of certain modern theorists?

Prof. Barrett's article, "The Jubilee Singers," is objectionable as favouring by insinuation the vulgar belief that the distinction between the Aryan and the Ethiopian "races" lies in the colour of the skin,—a belief actually dangerous, since it leads some who should know better to apply the insulting and resented epithet "niggers" to our Hindoo fellow-subjects, or to include them along with the negro under the cant phrase "people of colour."

"Reminiscences of Holland" is a series of papers highly laudatory of the Dutch. Holland—by affording, as she once did, an asylum for persecuted thinkers, and a place where their works might be printed without the interference of the censorship—has merited well of the world. But there is a terrible set-off: she originated national debts — and chicory! The "Orthodox" party in Holland, a body somewhat kindred to the English "Evangelicals," sum up all the results of modern thought as "the lies," thereby not merely pronouncing it erroneous, but branding its advocates with insincerity. This reminds us of Prof. J. W. Dawson, who, in one of his invectives against the new Natural History, speaks of those who "affect" to be unable to recognise marks of design in the organic world. Does the learned Professor find insult sometimes a more convenient weapon than argument?

The reports on the "Progress of Science," including physics, geology, biology, chemistry, geography, and astronomy, are ably and fairly compiled.

We have especial pleasure in recognising the candid manner in which the doctrine of Evolution is handled. It is time the outside public made the discovery that there is nothing necessarily atheistic in the idea of a transmutation of organic forms.

Improvement of the Condition of Workmen. By T. Egleston, Ph.D. Philadelphia: Sherman and Co.

"Improvement of the condition of workmen," but how? High wages have been tried, with the simple and unsatisfactory result that the money is carried straightway to the tavern. Reduced hours of labour have tended greatly to the promotion of dog-races, pigeon-matches, and even to the revival of cock-fighting. Arrangements for mere physical welfare, such as are carried out at the Hôtel Louise, of the Hassard Collieries, near Liege, are, as Prof. Egleston remarks, only "insignificant items, so far as the general well-being of the men is concerned. Something requires to be done to elevate their moral as well as physical condition." But what is this something? The author describes the arrangements he has found in operation in different countries of Europe with different degrees of success. On returning to

America he hoped to put some of these schemes into practice, and "commenced, with more enthusiasm than judgment, to apply a system drawn from my experience in England and the Continent to Celts, with not very fortunate results. They were suspicious of the houses that I built; were certain that I was personally deriving benefit from the fines that I proposed for the foundation of their fund; were jealous of their own members, raised to higher position than themselves, as they thought, by being brought more in contact with the officers of the Company than they were; and caused so much trouble that in the interest of the Company I was obliged to abandon any attempts to elevate the condition of the men, and I must confess that I have been obliged as conscientiously to abstain from acting as I was at first disposed conscientiously to act."

This is a very discouraging result, and the suspicions of the men were no doubt ridiculous. Yet we must remember that the hated "truck system," which has enriched so many contractors and the like, has often been disguised under attempts to promote the welfare of the employed. Many judicious politicians even think that all connection between the employer and his workmen beyond the actual hours of labour should, on this account, be discouraged as much as possible by stringent legal enactments. Just as there are men whose only endeavour is to do their work as badly as they dare, and get through the week with the minimum amount of exertion, physical or mental, so there are masters who regard their men as natural enemies, to be outwitted and cheated if possible. Yet for a number of men to act successfully in concert without mutual respect and goodwill is in the long run impossible. In industry, as in war, the most perfect *matériel* is of little avail if the *personnel* be untrustworthy. Confidence between employer and employed, based on a conviction that either party is desirous to do his best for the other, is the first step towards an improvement in the condition of workmen; but there is no receipt for creating such confidence. The man who can inspire it, like the true poet, *nascitur, non fit.*

Building Construction, showing the Employment of Brickwork and Masonry in the Practical Construction of Buildings. By R. Scott Burn. London and Glasgow: W. Collins, Son, and Co.

The present work, otherwise unimpeachable of its kind, is strangely included in an "Advanced Science Series,"—an instance of a confusion very annoying to the methodologist, but to which the British public seem incurably given. The author treats on plan-drawing; on the employment of brick in construction; on the varieties of stone-work; foundations, walls,

arches, sewers, drains, tanks, and wells; on concrete building, &c. The work is profusely illustrated, and will be of great value to those engaged in the study of the constructive arts.

The State of the Medical Profession in Great Britain and Ireland. By WILLIAM DALE. Being the successful Carmichael Prize Essay in 1873. Dublin: J. Atkinson and Co.

FROM the Preface to this Essay we have great pleasure in extracting the following passage:—

"Certain restless women have sought recognition as medical probationers or students, of the different corporations; especially have they, with wonderful persistence, forced their so-called claims upon the attention of the University of Edinburgh and the General Medical Council. The former of these bodies has emphatically said 'No' to these bold aspirants after medical work and medical fame; but the latter, by the half-hearted mode in which it has dealt with the question, has opened the door to future trouble, and given grave offence to the profession. A short time ago, at Heidelberg, we saw two young female students going round the wards of the hospital with the young men. The strange sight arrested instant attention, but the exhibition sadly repaid it. They stood behind the male students, and seemed to have no place in the class; indeed how could they? Look at the case under observation. It is a case of Ascites in a man! What could gallantry, or good nature, or anything else do for them? Where could they be placed but behind?"

If we look further into the pamphlet, and note how overcrowded the profession evidently is, we shall yet further question the wisdom of those who seek to assist women in obtaining medical qualifications.

The following account is true, and "pity 'tis 'tis true":— "We see men with diplomas keeping open retail shops, and selling almost everything that the druggist sells, hair-oil, scented soap, perfumery, tobacco and cigars, patent medicines, &c. We see others having open surgeries, one remove above shops, with their glaring red lamps, as trade-marks to attract customers. We see others jostling each other in the strife for existence, advertising and underselling like any Cheap Jack of the day; seeking to visit patients and supply medicines for 1s. 6d., 1s., or even 6d. In a shop-window not more than a stone's throw from Westminster Abbey the writer saw medical attendance and medicine advertised at even a lower rate than this." The Medical Club system and the Poor-Law service give equally convincing proofs of the unsatisfactory state of the medical profession,—a state as dangerous to the public as it is degrading to the practitioner. The new sphere of sanitary work

created by the Public Health Act has scarcely, as our author hopes would be the case, brought "fresh emolument and honour to the profession." In some districts, where there is abundant work, the "Medical Officer of Health" receives a salary of £10 yearly, on the understanding that he is not to interfere with any source of disease and death.

Mr. Dale's remedy for the repletion and consequent degradation of the profession is that the present multiplicity of examining and licensing boards should cease, and that there should be only one portal through which all should enter, and that under more stringent regulations. The author's proposals deserve careful consideration, and would be as a whole beneficial, though we cannot endorse them in detail.

Blue- and Sun-Lights; their Influence upon Life, Disease, &c. By General A. J. PLEASANTON. Philadelphia: Claxton and Co. London: Trübner and Co.

WE have here a book printed in blue ink, bound in blue, and treating on the chemico-physiological action of the blue ray of light. The author's fundamental experiment is as follows:—A vinery was built in 1861. Its dimensions, situation as regards the points of the compass, &c., are minutely described, and present nothing remarkable. But every eighth row of glass in the roof was of violet glass, the rows on opposite sides alternating. The mould had no peculiarity, and the vines, of twenty different sorts, were planted in the usual manner in the month of April. In the early part of September, the same year, some of the vines were found to be 45 feet in length, and an inch in diameter at a foot from the ground, whilst similar vines, planted about the same time, had only reached the length of 5 feet. In September, 1862, the vinery yielded 1200 lbs. of grapes. The remark is added that "in grape-growing countries a period of from five to six years will elapse before a single bunch of grapes can be produced from a young vine." The next season (1863) the yield of grapes was 2 tons, the vines being perfectly healthy and free from disease. "So on, year by year, the vines have continued to bear large crops of fine fruit without intermission for the last nine years. They are now healthy and strong, and as yet show no signs of decrepitude or exhaustion."

Similar experiments have been made on poultry, pigs, and on a calf, and in all cases the result is described as being analogous —accelerated maturity, increased size, weight, and vigour.

Now we do not for a moment wish to accuse General Pleasanton of trifling with the public, and we can scarcely see how the enthusiasm to which all discoverers are liable can have led

him astray in matters which are determined by weight and measure. His experiments, too, are very easily capable of verification. Still we are bound to say that his results accord very ill with those of other and earlier experimentators. Thus Prof. H. Vogel—certainly no mean authority on the chemical action of light—states, in his "Chemistry of Light and Photography" (p. 78), that "recent observations have established that the yellow and red rays, and not the blue and violet, produce the greatest chemical effect on the leaves of plants." Dr. R. Hunt, in that well-known work the "Poetry of Science," fully admits that seeds under blue glass will germinate long before others exposed to ordinary daylight, whilst under the yellow ray the process of germination is entirely checked; but he resumes—"If the experiment is continued it will be found that under the blue glass the plants grow rapidly, but weakly, and that, instead of producing leaves and wood, they consist chiefly of stalk, upon which will be seen here and there some abortive attempts to form leaves. When the process of germination has terminated, if the young plant is brought under the yellow glass it grows most healthfully, and forms an abundance of wood, the leaves having an unusually dark green colour, from the formation of a large quantity of chlorophyl. Plants do not, however, produce flowers with readiness under this medium; but if, at the proper period, they are brought under the red glass, the flowering and fruiting processes are most effectively completed."

In confirmation Dr. Hunt quotes a letter from Mr. C. Lawson, of Edinburgh, an eminent seed-merchant. This gentleman, as early as 1853, had proved the value of blue light in accelerating germination, and employed it practically in testing the value of the seeds coming into his hands in the course of business. He found that seeds could be thus caused to germinate in two to five days, instead of, as heretofore, in eight to fourteen; but he adds that he has "always found the violet ray prejudicial to the growth of the plant *after* germination."

Here, then, is a complete discrepancy, and either General Pleasanton on the one hand, or Messrs. Vogel, Hunt, and C. Lawson on the other, must be decidedly mistaken. In all such cases the only expedient is further experiment, which in this case certainly involves no delicate manipulation, and might be satisfactorily performed by any intelligent horticulturist. One point of difference between General Pleasanton's arrangements and those of the European experimentators upon the influence of the various rays of light upon organic life is, that the latter—ourself included—submitted plants and animals to the sole and exclusive action of blue, yellow, or red light respectively, whilst in General Pleasanton's experiments the blue light has been used mixed in certain proportions with ordinary daylight.

It appears that the author applied for a patent for his discovery, and that Prof. Brainerd was deputed by the Commissioners of

Patents to investigate the reality of the alleged results. After a minute inspection he is reported to have declared that all General Pleasanton's statements concerning the action of blue light were confirmed.

But if General Pleasanton is in the right, the wonderful and salutary effects of blue light upon organic life is by no means the most extraordinary of its properties. Heat is also, in some unaccountable way, developed in the passage of sunlight through blue glass. We read—"During the winter of 1871-72, which in this city was a very cold and rigorous one, two ladies of my family, residing on the northern side of Spruce Street, east of Broad Street, in this city,—who, at my suggestion, had caused blue glass to be placed in one of the windows of their dwelling, associated with plain glass,—informed me that they had observed that when the sun shone through these associated glasses in their window, the temperature of the room, though in mid-winter, was so much increased that on many occasions they had been obliged during sunlight to dispense entirely with the fire which ordinarily they kept in their room, or, if the fire was suffered to remain, they found it necessary to lower the upper sashes of their windows, which were without the blue glass, in order to moderate the oppressive heat." Further, a "distinguished German scientist," not named, is quoted as declaring that "one-half of the fuel heretofore required can be saved by thus utilising sunlight." Yet in an earlier portion of the work we find the author expressing the opinion that a vinery roofed with blue glass would be cooler than one constructed of uncoloured glass. "Should too much (blue glass) be used it would reduce the temperature too much." It need scarcely be said that experimentalists have not found the blue and violet rays of the spectrum to be the hottest portions.

We should feel much greater confidence in General Pleasanton's observations if he had been content to place them simply before the world as novel and—if verified—important facts; but he goes much farther, and deduces from them an entire new philosophy. Into these his doctrines it will be early enough to examine when the action of blue light shall have been satisfactorily ascertained.

Hydraulic Experiments at Roorkee, 1874-5. By Capt. Allan Cunningham, R.E. Roorkee: Thomason College Press.

An experimental examination of surface and sub-surface velocities in rivers, flowing canals, &c.

A Course of Practical Chemistry arranged for the Use of Medical Students. By W. ODLING, M.B., F.R.S. Fifth Edition. London: Longmans and Co.

THIS is a new and improved edition of Dr. Odling's well-known treatise on chemical manipulation, qualitative analysis, toxicology, and animal chemistry. The former portion has been carefully revised by Dr. John Watts, and the latter by Dr. T. Stevenson, of Guy's Hospital.

At the present time it is more than ever imperative upon medical students to make themselves perfectly acquainted with the action of poisons upon the animal system, and to acquire at least a rudimentary knowledge of toxicology. Taking its cue from certain recent cases of a somewhat sensational character, a portion of the lay press is labouring assiduously for a triple object—to create a panic on the subject of secret poisoning; to proclaim, by implication at least, eminent medical and chemical authorities as deficient in their knowledge of the symptoms and the behaviour of poisons; and to demand increased restrictions upon the sale of substances of a deleterious character. It is darkly hinted that were inquiries as careful as that in the Balham case more common, many deaths now accepted as "natural" would be removed into another and more alarming category. It is insinuated that physicians cannot or do not distinguish the symptoms of natural disease from the effects of poisons, and that chemical analysis is in complicated cases not to be depended on.

We cannot accept these views. For the frequency of secret poisoning there is no proof, but merely the random surmise of "smart writers" wishful to create a sensation in the so-called "silly season." As regards the alleged incompetency of men of science, it must be remembered that it was not the medical and chemical practitioners who broke down in the "Balham mystery." The medical experts did not fail to recognise in the symptoms exhibited by the patient the effects of an irritant poison. The chemist detected that poison as present in a fatal quantity. If, therefore, a failure of justice has here occurred,—which it is not our province to examine.—it is surely idle in the extreme to make of the Bravo tragedy a peg on which to hang scandalous charges against men of science.

As to increased restrictions upon the sale of poisons, we must—in the name both of chemical research and chemical manufactures—protest against such a proposal. If we are to banish from the workshop every substance which cannot be swallowed with impunity, our trade will indeed be imperilled. Not only are deadly drugs in constant and necessary use in the factory, but poisons of the most formidable character can be gathered in wood, field, and garden. Further, even if all poisons could be kept out of the hands of malicious persons, nothing would be gained. Of what use is it, as Milton asks in his "Samson

Agonistes," to guard one gate of a city and leave another open to the enemy? What is the use of severe "Pharmacy Acts" as long as explosives and fire-arms may be purchased and kept by all sorts and conditions of men? It is all very well for a daily paper to indulge in the statement that "an ounce of cyanide of potassium is capable of doing more harm than a ton of nitro-glycerine." Even were this true, the writer overlooks the main point—that explosives can be used to produce such sweeping havoc as the Bremerhaven and the Clerkenwell outrages, whilst even the deadliest poison, in the hands of the most remorseless villain, can prove fatal only to those few persons to whose food he has access; a fact which terribly narrows the circle within which the guilty person must be sought. We must therefore denounce, as utterly uncalled for, the proposal to increase the stringency and the scope of the Acts regulating the sale of poisons. It is a great misfortune that when political and literary organs find themselves compelled to deal with some scientific topic, they do not see the propriety of putting the matter into the hands of some specially qualified writer.

Annual Record of Science and Industry for 1875. Edited by SPENCER F. BAIRD. London: Trübner and Co.

WE have here a condensed summary of the most important inventions, discoveries, and observations made during the year 1875.

Among the paragraphs thus collected we are first struck, though most unfavourably, with a "new mode of embalming," said to have been discovered by one Madame Jalourear. The corpse is to be placed in an impermeable coffin, together with certain substances which produce a rapid, though not putrid, decomposition, whilst the products cannot escape from the enclosure. The substance in question is phosphate of lime, which is said to have the property of causing rapid decomposition without offensive odour. To begin with, we doubt this very much. We have always found soluble phosphates—and no less insoluble ones, if in a gelatinous state—powerful promoters of putrefaction in its most offensive form, whence the necessity, in the treatment of sewage, of withdrawing all phosphates from solution. It is further a grievous fault that all the nitrogen, phosphates, and other principles found in the bodies of the dead should be withdrawn from natural circulation, and that an additional quantity of phosphate of lime should be wasted in the attempt to modify the character of decomposition. The scheme holds, in our opinion, a doubtful position between folly and crime.

In a paper on the influence of the roots of living vegetables upon putrefaction, Jeannel pronounces "the fact that cemeteries, bogs, and marshes are made salubrious by vegetation indisputable, being purely the result of experience." Would M. Jeannel kindly show how this theory adapts itself to such districts as the Terai, the Gold Coast, the Tierras Calientes of Mexico, &c., where we have at once the most luxuriant vegetation and the most intense insalubrity?

The intellectual capacity and educability of children of different "races"—to say species would be, we suppose, a capital offence—has been studied by M. Houzeau. He concludes that there is in each child a different rate of intellectual proficiency, but that such differences amount to less than might be anticipated, and that an unequal rate of improvement does not belong specially to any one race. Nay, as we have seen it remarked elsewhere, the inferior races even seem to have an advantage. On this fact, however, the very just comment has been made by other observers that this advantage is extremely transitory.

The existence of gigantic cuttles is now no longer a matter of question. One, whose larger arms measured each 26 feet in length and 16 inches in circumference, was cast ashore on the Grand Bank, Fortune Bay, in December, 1874. The entire length of its body was 14 feet.

As another wonder of the deep may be mentioned a marine worm 300 yards in length, discovered by Dr. Carl Möbius, off Mauritius. Is not this quite as extraordinary as the much-disputed "sea-serpent"?

Closet naturalists have sometimes maintained that man has at all times and in all places, except where debarred by climate, selected the same animals for domestication, the inference being drawn that no others were capable of being truly and permanently tamed. This theory is disproved by evidence obtained from Egyptian monuments. Several species of antelope were formerly bred and kept in a state of domestication, such as the gazelle, the kobe, addax, and beisa. In the pictures on Egyptian tombs "flocks of these animals are represented receiving the attentions of the farmer and the herdsman." From about 1800 B.C. these representations become fewer and fewer in number, and ultimately disappear. It would be interesting to ascertain whether their removal from the ranks of tame animals was due to the conclusion that they were not remunerative, or to a general decline in the art of husbandry.

It has been justly remarked that man's power of destroying noxious animals, from the tiger or the cobra down to the locust and the mosquito, has increased in a far less rapid ratio than his means of inflicting death and destruction upon his own species. In the former department there is indeed great room for inventive ingenuity. Two species of *Pyrethrum* (*P. carneum* and *roseum*) have been for some time in use for destroying, or at least

banishing, noxious insects, under the name of "Persian powder." The *P. cinerariafolium*, a Dalmatian species, is now found to be more active, and is coming into extensive use. We have been informed, however, that—as far at least as gnats and mosquitoes are concerned—the marsh rosemary (*Ledum palustre*) is more effectual than any species of *Pyrethrum*. As the *Ledum* flourishes in all boggy parts of Central and Northern Europe, and, if we are not mistaken, of Labrador, Canada, and the Hudson's Bay countries, the supply may be considered unlimited. It has been also alleged that the common brake fern (*Pteris aquilina*) is a nuisance to all insects. This we cannot admit, at least when the plant is in its recent state. We see butterflies and moths resting upon it; we have found its leaves eaten by caterpillars, and we never observe that those parts of woods where it is most luxuriant are less infested with flies than localities where it is absent.

An interesting observation, made by Professors Lartet, Gervais, Cope, and Marsh, is that the brains of extinct Mammalia have evidently been much smaller than are those of the most closely analogous species at the present day. "In the lines of the rhinoceros, tapir, and horse, a regular increase in size from such beginnings can be traced."

It would be alike impracticable and unfair were we to extend further our quotations, but we think the few specimens we have given will be amply sufficient to show the interesting character of the work.

Science Papers, chiefly Pharmacological and Botanical. By DANIEL HANBURY, F.R.S. Edited, with Memoir, by JOSEPH INCE, F.L.S., F.C.S., &c. London: Macmillan and Co.

PHARMACY, high as its claims may fairly be pronounced, is by no means popular. It can point to none of those triumphs which so forcibly appeal to the public imagination, such as the solution of phosphates, the discovery of the coal-tar colours, or the invention of malleable glass; nor is it connected with any of those dazzling speculations which are so enthusiastically welcomed by the young, however their foundations may fail to satisfy the old. It has to rely for appreciation upon sound, conscientious, valuable work, which attracts little notice from those whom it benefits. We need not wonder, therefore, that the author of these papers and the subject of this memoir is little known to the British public. Daniel Hanbury was not one of the class—unhappily now so common, and, more unhappily still, so much fostered by our institutions—who will, at a few days' notice, "get up" any subject, and write about it a showy superficial dissertation. He was no writer of "Manuals," repeating

for the twentieth time fully-recognised facts, or perhaps current errors. He was a specialist who, confining his attention to what some, perhaps, may deem a limited sphere, was within that sphere satisfied with nothing short of perfection. Whatever he did bears the royal stamp of thoroughness. Never was he content with hearsay information if by any amount of labour it was possible to go to the fountain-head. As his friend and fellow-labourer Professor Flückiger observes,—" In countless instances second-hand knowledge could not stand its ground before his critical acumen, and had to give way before his superior observations. Only the most trustworthy reports, samples, and specimens collected with the greatest care by a skilful hand satisfied him. The best book-knowledge offered for sale in the market did not content him, but he referred back to the sources of information, testing them minutely." These attributes appear most strikingly manifested in the " Pharmacographia," the joint work of Flückiger and himself, a compilation which—" from the amount of its original matter, its laborious verification of facts, the accuracy of its references, and the extent of general erudition it reveals "—is perfectly unapproachable. It is to be regretted that a career so fruitful in the choicest results should have been brought to so early a close.

The volume before us includes a memoir of the deceased *savant*, his scientific papers, his addresses and miscellaneous papers, and an obituary notice from the pen of Dr. Flückiger. The editor has performed his task well, though we regret to find that he embraces and advocates the dangerous error that a life of business can be at the same time a life of scientific research. To this view we may refer elsewhere.

The Chemistry of Light and Photography in their Application to Art, Science, and Industry. By Dr. Hermann Vogel. London: H. S. King and Co.

This edition of Dr. Vogel's work is undoubtedly an improvement upon the foregoing. Most of the errors, chemical and otherwise, which disfigured the first edition have disappeared, and the English editor may be fairly congratulated upon having done his part satisfactorily.

But with the work itself we are not perfectly satisfied. It is certainly a valuable treatise upon photography, but the chemistry of light is here, in our opinion, very insufficiently treated. The title naturally leads us to expect that the behaviour of all known classes of bodies, natural and artificial, under the influence of light, and the effects of the solar rays in promoting or hindering combinations and decompositions, would have been at any rate briefly noticed. But into the chemical and bio-chemical action

of light the author scarcely enters. Nay, in some cases, the few remarks which he makes on such subjects are scarcely fairly representative of the present state of science. Thus we read :—"The green colour of leaves and the variegated colours of flowers exist only under the operation of light. In the dark, plants only develop sickly blossoms, like the well-known white sprouts of potatoes kept in cellars."

The views here expressed are much too sweeping, and require to be modified. Thus Dr. Askenasy finds that the influence of light upon the colours of flowers varies greatly in different plants. The flowers of different varieties of *Tulipa Gesneriana*, whether yellow, scarlet and yellow, or scarlet and white, when grown in complete darkness, displayed no constant and appreciable difference in colour or intensity from flowers of the same respective kinds reared in the full light. The flowers of the common spring crocus, both blue and yellow, appeared in their natural colours, though not well developed. Upon other flowers, such as a dark blue hyacinth, the absence of light was found to produce more marked effects. Flowers grown in the light, though at the same temperature, were fully a fortnight earlier than such as were grown in darkness, and were more intensely coloured. Still the latter were not absolutely colourless. Thus it appears that the influence of light upon the development of vegetable and animal colouring matters, not to speak of other principles, is a subject which requires much more attention than it has yet received, and can by no means be cursorily dismissed. One of Dr. Askenasy's experiments is exceedingly interesting. Portions of the imperfectly coloured flower-spikes of several blue hyacinths, growing in the dark, were cut off and placed in glasses of water exposed to the full light on the south side of a greenhouse. In a few days these flowers had attained as full a shade as those which had been reared in full daylight from the first; which proves that the development of colour does not depend on a previous formation of chlorophyll.

These deficiencies are the more to be regretted because Dr. Vogel, in his Preface, if we do not misunderstand him, complains that "men of science have in great measure neglected this subject after the first enthusiasm."

On the other hand, some amount of space is taken up with matter which might well have been omitted. Thus we find an explanation of what is meant by the term "elements," which, now manuals of chemistry have been multiplied to such an extent, can scarcely be regarded as necessary.

The following passage gives room for reflection :—"Equally peculiar are the changes experienced in sunlight by two other elements not so well known, chlorine and bromine, which have only been carefully observed latterly." Chlorine and bromine, we should consider, may rank among the best known elements, and their combination with hydrogen gas under the action of

sunlight—which, from the context, appears to be the peculiar change here referred to—has been carefully observed some time ago.

On the other hand, what may be called the strictly photographic portion of the work is very valuable.

The chapter on the "correctness of photographs," in which the author treats of the individuality of the photographer, of the influence of lenses, of the length of exposure, of colours and models, of the characteristic feature in the picture, of deviation from truth in photography, and of the difference between photography and art, may be read with great advantage by photographers, artists, and amateurs. Prof. Vogel does not countenance the popular notion that a photograph is necessarily and invariably more correct than a picture. We think that it is Nathaniel Hawthorn who maintains that the character of a person is more truly shown in his photographic likeness than in his face. That this may not have been incidentally the case we are not prepared to deny; but it is certainly not the rule. On the contrary, photography is apt to flatter one class of persons, and in revenge to be less than just to another. Among the latter are often found those whose principal attraction depends more on the play of expression than upon strict regularity of feature.

The author figures and recommends a photometer for determining the exact time needful for exposure. It will doubtless be evident that for this purpose the radiometer will probably prove highly valuable. The chapter on "photography in natural colours" gives an account of certain attempts made to solve this interesting problem, and recalls to our memory a strange hoax successfully perpetrated many years ago. It was announced that the art of taking coloured photographs had been discovered in America, and a descriptive catalogue of the specimens on their way for exhibition in London "went the round of the papers." But the specimens never arrived, and the whole affair proved to be merely a most unjustifiable falsehood.

The concluding chapter, on photography as a subject to be taught in industrial schools, is worth the serious attention of educational boards. Of course the term "industrial schools" is used here in its natural acceptation, and not in the strangely perverted meaning which has been forced upon it in England within the last few years.

The Principles of Dynamics; an Elementary Text-Book for Science Students. By R. WORMELL, D.Sc. London, Oxford, and Cambridge: Rivingtons.

THE author remarks, in his Preface, that "the object of the present work is to present and unfold the subject according to

the method best suited for the purpose of the science student. It aims at supplying, without the aid of advanced mathematics, such explanations of the Laws of Dynamics as will prepare the way for their application to physical phenomena, particularly to those of Heat and Electricity."

In carrying out his undertaking, the author treats in succession of time and space, motion, force and motion, force and mass, momentum and energy, the laws of motion, the parallelogram of forces, moments of forces, reactions and surfaces, centres of gravity, energy, a system of particles, problems on energy, machines, friction, and moments of inertia. The descriptions and explanations are necessarily brief, but exceedingly lucid.

The work is plentifully illustrated with diagrams, and a series of questions is appended to every section. It may be safely recommended to students.

Check List of the Ferns of North America, North of Mexico. Published for John Robinson, Salem, Mass. The Naturalists' Agency.

This pamphlet is simply a Catalogue of all the ferns known to be indigenous in the United States and the Dominion. The total number of species is remarkably small in relation to the vast extent of territory. British fern collectors and cultivators will be interested to find that many of their old friends—such as *Osmunda regalis*, *Phegopteris dryopteris*, *Polypodium vulgare*, *Scolopendrium vulgare*, &c.—are common to both sides of the Atlantic.

Notes on Building Construction, arranged to meet the Requirements of the Syllabus of the Science and Art Department of the Committee of Council on Education. Part II.—Commencement of Second Stage, or Advanced Course. London, Oxford, and Cambridge: Rivingtons.

This second part treats of brickwork and masonry, timber roofs, roof-coverings, of beams and girders, of centres, joinery, stairs, rivetting fire-proof floors, iron roofs, plasterer's work, and painting. We should think that the artizan who has fairly mastered the contents of this work—we do not say who is able to "pass" in it—will very soon find the advantage, and will prove decidedly superior to those of his companions who are content to pass through life without caring at all for the fundamental

principles of their work. The time has gone by when that mere mindless routine which some men call "practice" can suffice in any art. In the chapter on "graining" we regret to find that this national vice passes without a protest. It is not art, but simply falsehood, for which no rational plea can be urged.

Whilst pronouncing this book highly valuable to all connected with any of the building trades, we cannot help asking whether all other branches of industry are about to be dealt with as thoroughly and carefully, and if not, why not?

It is somewhat peculiar that the title-page does not bear the name of any author or editor.

Field Geology. By H. PENNING, F.G.S. (H.M. Geological Survey of England and Wales). *With a Section on Palæontology.* By A. J. JUKES-BROWNE, F.G.S. (H.M. Geological Survey). London: Baillière, Tindall, and Cox.

EVEN in this age, so rich in manuals, hand-books, and text-books, it will happen that a science, under some very important aspect, may escape attention. Geology certainly has now been popular for a long time; its cultivators are numerous and increasing, and its literature is most comprehensive. Looking over the formidable array of published books, we might at first sight imagine that the subject had been already treated from every conceivable point of view. We have works theoretical and works practical, works elementary and works advanced, and yet till the appearance of the little volume before us one phase of geology appears to have been overlooked. Let us suppose a student who has made himself familiar with the science by reading, and wishes, as every student should, to become himself an observer. How is he to proceed? He has read of green-sand, of oolitic limestone, of Kimmeridge clays, and the like, but the ordinary hand-books give him no clue as to how he is to know these and other rocks when he meets them. Here Mr. Penning steps in, and teaches him how and what to observe.

The author describes actual geological work under four heads —mapping, sections, the determination of rocks (lithology), and the observation of fossils (palæontology), and for each of these simple and practical instructions are given, the student being merely pre-supposed to have a fair general knowledge of the science, of the sequence of the various systems, formations, and groups, and of the general succession of fossil plants and animals. As far as possible the directions given are illustrated by examples of their practical applications. The section on the determination of rocks is exceedingly valuable. The author, from his extensive practical experience, is able to propose those

methods which can be carried out in the field with the minimum of appliances. The student who follows his directions, and who has duly prepared himself by a careful study of cabinet specimens of the ordinary rocks and minerals, will soon feel at home in this department of geology. The author reverses the process often directed; he tabulates the results obtainable, and from them deduces the nature of the rock in question. The examination to which he subjects rocks in the field consists of noting the texture, fracture, lustre, behaviour with knife, effervescence with dilute acid, colour, and streak, the inferences drawn from these indications being afterwards verified at home by a determination of the hardness, specific gravity, solubility, and behaviour before the blowpipe. The examination of thin slices of rock under the microscope—of course impracticable in the field—is strongly and very justly recommended.

In the section on palæontology—the first three chapters of which are from the pen of Mr. Jukes-Browne—we meet with a remark which we would commend to the attention of those who are given to complain of the absence of "missing links:"—"Since the majority of rocks composing the crust of the earth are of aqueous and principally marine origin, we should naturally expect the fossils they contain to be the remains of aquatic and principally marine beings. Thus, in the Vertebrata, the remains of mammals and birds are among the rarest of geological relics, even in beds of littoral or terrestrial origin, save those of very recent date." This plain simple truth fully exposes the absurdity of drawing any conclusions from the absence of this or the other form of extinct life in the strata which we have been able to examine. The instructions given for collecting fossils bear the marks of experience and sound sense. The following "wrinkle" may be of value to the student:—"Chalk-fossils, and those which have been obtained from any similar porous limestone along the *sea-shore*, should be soaked in fresh water for several weeks, the water being changed at least once a week: this is the only way we know of to prevent the efflorescence of the salt in such cases, and the consequent splitting up of fossils which have cost time and pains to extract."

We find some very just remarks on the naming and arranging of fossils. The author laments the illusory ideas regarding the fixity of species, and the too great desire of finding something new to science "which too often takes the form of *species-making* instead of *discovery*." For this tendency he considers that "the only cure is to read Darwin's 'Origin of Species,' and persons who will not do that may be given up as hopeless."

We can decidedly recommend this work to the student as supplying a want of no mean importance.

PROGRESS IN SCIENCE.

THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE British Association has just concluded a highly successful meeting at Glasgow. The local arrangements were all that could be desired, while many of the papers read were of great scientific value. The number of members and associates present at the meeting was 2731.

In his Inaugural Address, the President—Dr. Andrews, F.R.S.—first gave a masterly review of the progress of science, but while he alluded at considerable length to the results obtained by scientific men in England, America, France, Germany, Italy, and Russia, he made too slight a reference to his own brilliant physico-chemical researches, which have added so much to our knowledge of the laws and composition of gases, to the nature and properties of ozone, and to the heat changes in chemical reactions, &c. In speaking of the North Polar Expedition, he referred to the opinion of those who hold that a full survey of the Arctic regions can never be of such value as to justify rhe risk and cost which must be incurred, and said that it was not by such cold calculations that great discoveries were made or great enterprises achieved. There was an inward and irrepressible impulse—in individuals called a spirit of adventure, in nations a spirit of enterprise—which impelled mankind forward to explore every part of the world we inhabit, however inhospitable or difficult of access; and if the country claiming the foremost place among maritime nations shrunk from an undertaking because it was perilous, other countries would not be slow to seize the post of honour. If it were possible for man to reach the poles of the earth, whether north or south, the feat must sooner or later be accomplished; and the country of the successful adventurers would be thereby raised in the sale of nations. He then alluded to the transit of Venus; to the confirmation by M. Cornu of Foucault's calculation of the distance of the earth from the sun; to Mr. Christie's confirmation of Dr. Huggins's discovery that some of the fixed stars are moving towards and some receding from our system; to Mr. Stone's confirmation of Prof. C. A. Young's observation that bright lines, corresponding to the ordinary lines of Fraunhofer reversed, may be seen in the lower strata of the solar atmosphere for a few moments during a total eclipse; to the observations of Roscoe and Schuster on the absorption bands of potassium and sodium; to Mr. Lockyer's investigation on the absorbtive powers of metallic and metalloidal vapours at different temperatures; to M. Lecoq de Boisbaudran's discovery of the new metal—gallium; to the discoveries and researches of Sir Edward Sabine, Nordenskiöld, Maskelyne, Lawrence Smith, Sorby, R. Apjohn, Daubrée, Wöhler, and Tschermak in connection with meteoric science.

In noticing the important services which the Kew Observatory has rendered to meteorology and to solar physics, Dr. Andrews expressed the hope that England would not lag behind in providing physical observatories on a scale worthy of the nation and commensurate with the importance of the object.

After a reference to the organisation in this country of a system of reporting by telegraph the state of the weather at selected stations to a central office, so that notice of the probable approach of storms may be given to the seaports, and also to Dr. Robinson's observations at the Observatory of Armagh, which led to the discovery that the mean velocity of the wind is greatest in the S.S.W. octant and least in the opposite one, and that the amount of wind attains a maximum in January, after which it steadily decreases with one

slight exception, till July, augmenting again till the end of the year, Dr. Andrews passed to the subject of electricity; he announced the failure of a recent attempt to deprive Oerstedt of his great discovery. To Franklin, Volta, Coulomb, Oerstedt, Ampère, Faraday, Seebeck, and Ohm he ascribed the fundamental discoveries of modern electricity. In its applications the names of Davy, Wheatstone, Morse, and Thomson are prominent. In connection with the theory of electrical and magnetic action we find the names of Poisson, Green, Gauss, Weber, Helmholtz, Thomson, and Clerk Maxwell. Among recent inventions is mentioned Prof. Tait's discovery of consecutive neutral points in certain thermo-electric junctions; De la Rue and Müller's chloride of silver battery giving freely sparks through cold air, which, when a column of pure water is interposed in the circuit, accurately resembles those of the common electrical machine. The length of the spark increasing nearly as the square of the number of cells, it has been calculated that with 100,000 elements of this battery the discharge should take place through a distance of no less than eight feet in air.

Reference was made to Newton's grand investigation of the properties of light; to Lord Rosse's discovery that the surface of the moon facing the earth passes, during every lunation, through a greater range of temperature than the difference between the freezing and boiling points of water; to Prof. Stokes's discovery of the cause of epibolic dispersion, in which he showed that many bodies had the power of absorbing rays of high refrangibility, and of emitting them as luminous rays of lower refrangibility; to Mr. Crookes's experiments on repulsion caused by radiation; and to the discovery by Mr. Willoughby Smith of the power of light in diminishing the electrical resistance of selenium. This property has been ascertained to belong chiefly to the luminous rays on the red side of the spectrum. Lord Rosse observed that the action appeared to vary inversely as the simple distance of the illuminating power, and the accuracy of this observation has since been confirmed by Prof. W. G. Adams.

In welcoming General Menabrea as a distinguished representative both of the kingdom of Italy and Italian science, Dr. Andrews spoke of his great work on the determination of the pressures and tensions in an elastic system, the principle being stated in the following words:—"When any elastic system places itself in equilibrium under the action of external forces, the work developed by the internal forces is a minimum." He then referred to the mechanical integrator of Prof. J. Thomson, in which motion is transmitted, according to a new kinematic principle, from a disc or cone to a cylinder through the intervention of a loose ball; to Sir W. Thomson's machine for the mechanical integration of differential equation of the second order, and also to his tidal machine, by means of which the height of the tide at a given port can be accurately predicted for all times of the day and night.

The attraction-meter of Siemens was mentioned as an instrument of great delicacy for measuring horizontal attractions, which it is proposed to use for recording the attractive influences of the sun and moon, upon which the tides depend, and by means of Mr. Siemens's bathometer, in which the constant force of a spring is opposed to the variable pressure of a column of mercury, the depth of the sea may be approximately ascertained without the use of a sounding-line.

The President then remarked that it was often difficult to draw a distinct line of separation between the physical and chemical sciences; and it was doubtful whether the division was not really an artificial one. The chemist could make no large advance without having to deal with physical principles; and to Boyle, Dalton, Gay-Lussac, and Graham were due the discovery of the mechanical laws which govern the properties of gases and vapours. Some of these laws had of late been made the subject of searching inquiry, which had fully confirmed their accuracy, when the body under examination approached to what had not inaptly been designated the ideal gaseous state. But when gases were examined under varied conditions of pressure and temperature, it was found that these laws were only particular cases of more general laws, and that the laws of the gaseous state, as it exists in nature, although they

might be enunciated in a precise and definite form, were very different from the simple expressions which apply to the ideal condition. The new laws became in their turn inapplicable when from the gaseous state proper we passed to those intermediate conditions which, it had been shown, linked with unbroken continuity the gaseous and liquid states. As we approached the liquid state, or even when we reached it, the problem became more complicated; but its solution even in these cases might confidently be expected to yield to the powerful means of investigation we now possess.

Among the more important researches made of late in physical chemistry, those of F. Weber on the specific heat of carbon and the allied elements, of Berthelot on thermo-chemistry, of Bunsen on spectrum analysis, of Wüllner on the band- and line-spectra of the gases, and of Guthrie on the cryohydrates were mentioned.

Cosmical chemistry abounded in facts of the highest interest. Hydrogen, which, if the absolute zero of the physicist did not bar the way, we might hope yet to see in the metallic form, appeared to be everywhere present in the universe. It existed in enormous quantity in the solar atmosphere, and it had been discovered in the atmospheres of the fixed stars. It was present, and was the only known element of whose presence we are certain, in those vast sheets of ignited gas of which the nebulæ proper are composed. Nitrogen was also widely diffused among the stellar bodies, and carbon had been discovered in more than one of the comets. On the other hand, a prominent line in the spectrum of the Aurora Borealis had not been identified with that of any known element; and the question might be asked—Does a new element, in a highly rarefied state, exist in the upper regions of our atmosphere? Or are we with Ångström to attribute this line to a fluorescent or phosphorescent light produced by the electrical discharge to which the aurora is due? This question awaited further observations before it could be definitely settled, as did also that of the source of the remarkable green line which is everywhere conspicuous in the solar corona.

Here Dr. Andrews paid a tribute to the memory of Ångström, whose great work on the solar spectrum will always remain as one of the finest monuments of the science of our period. The influence, he said, which the labours of Ångström and of Kirchhoff had exerted on the most interesting portion of later physics could scarcely be exaggerated; and it might be truly said that there were few men whose loss would be longer felt or more deeply deplored than that of the illustrious astronomer of Upsala.

Passing to the application of science to the useful purposes of life, Dr. Andrews referred to the application by Neilson of the hot-blast to the smelting of iron. The Bessemer steel process and the regenerative furnace of Siemens were later applications of high scientific principles to the same industry. But there was ample work yet to be done. The fuel consumed in the manufacture of iron, as, indeed, in every furnace where coal was used, was greatly in excess of what theory indicated; and the clouds of smoke which darkened the atmosphere of our manufacturing towns, and even of whole districts of country, were a clear indication of the waste, but only of a small portion of the waste, arising from imperfect combustion. The depressing effect of this atmosphere upon the working population could scarcely be overrated. At some future day the efforts of science to isolate, by a cheap and available process, the oxygen of the air for industrial purposes might be rewarded with success. The effect of such a discovery would be to reduce the consumption of fuel to a fractional part of its present amount; and although the carbonic acid would remain, the smoke and carbonic oxide would disappear. But an abundant supply of pure oxygen was not now within our reach; and in the meantime he would suggest that in many localities the waste products of the furnace might be carried off to a distance from the busy human hive by a few horizontal flues of large dimensions, terminating in lofty chimneys on a hillside or distant plain. A system of this kind had long been employed at the mercurial mines of Idria, and in other smelting-works where noxious vapours were disengaged. With a little care in the arrangements, the smoke would

be wholly deposited, as flue-dust or soot, in the horizontal galleries, and would be available for the use of the agriculturist.

In speaking of organic chemistry, Dr. Andrews said that the discovery of quinine had probably saved more human life, with the exception of that of vaccination, than any discovery of any age; and he who succeeded in devising an artificial method of preparing it would be a true benefactor of the race. Not the least valuable, as it had been one of the most successful, of the works of our Government in India, had been the planting of the cinchona tree on the slopes of the Himalaya. As artificial methods were discovered, one by one, of preparing the proximate principles of the useful dyes, a temporary derangement of industry occurred, but in the end the waste materials of our manufactures set free large portions of the soil for the production of human food.

M. Dumas's method of destroying the *Phylloxera*, which lately threatened with ruin some of the finest vine districts in the South of France was then referred to. After a long and patient investigation, M. Dumas has discovered that the sulpho-carbonate of potassium, in dilute solution, fulfils every condition required from an insecticide, destroying the insect without injuring the plant.

The application of artificial cold to practical purposes was rapidly extending. The ice-machine was already employed in paraffin-works and in large breweries; and the curing or salting of meat was now largely conducted in vast chambers, maintained throughout the summer at a constant temperature by a thick covering of ice.

In completing his review, Dr. Andrews named the important work of Cayley on the "Mathematical Theory of Isomers," and to elaborate memoirs which had recently appeared in Germany on the reflection of heat- and light-rays, and on the specific heat and conducting-power of gases for heat, by Knoblauch, E. Wiedemann, Winkelmann, and Buff.

The latter part of the Address was devoted to the subjects of University Education and the Endowment of Research. A University, or *Studium Generale*, ought to embrace in its arrangements the whole circle of studies which involve the material interests of society, as well as those which cultivate intellectual refinement; and if, in accordance with the spirit of their statutes, or at least of ancient usage, the Universities would demand from the candidates for some of the higher degrees proof of original powers of investigation, they would, Dr. Andrews holds, give an important stimulus to the cultivation of science. The example of many continental universities, and among others of the venerable University of Leyden, was mentioned, and two proof essays recently written for the degree of Doctor of Science in Leyden —one by Van der Waals, the other by Lorenz, were referred to as works of unusual merit.

With regard to the endowment of research, Dr. Andrews refrained from discussing the subject as a national question, but considered that the universities ought never to be asked to give their aid to a measure which would separate the higher intellects of the country from the flower of its youth. It was only through the influence of original minds that any great or enduring impression could be produced on the hopeful student. Without original power, and the habit of exercising it, we might have able instructors, but we could not have great teachers. In every age of the world the great schools of learning have, as in Athens of old, gathered around great and original minds, and never more conspicuously than in the modern schools of chemistry, which reflected the genius of Liebig, Wöhler, Bunsen, and Hofmann. These schools had been nurseries of original research as well as models of scientific teaching; and students attracted to them from all countries became enthusiastically devoted to science, while they learned its methods from example even more than from precept. But while the universities ought not to apply their resources in support of a measure which would render their teaching ineffective, and would at the same time dry up the springs of intellectual growth, they ought to admit freely to university positions men of high repute from other universities, and even without academic qualifications. An honorary degree did not necessarily imply a university education; but if it had any

meaning at all, it implied that he who had obtained it was at least on a level with the ordinary graduate, and should be eligible to university positions of the highest trust.

Dr. Andrews advocated that the English universities should recognise the ancient universities of Scotland as freely as they had always recognised the Elizabethan University of Dublin. If this union were established among the old universities, and if at the same time a new university—as he had earnestly proposed ten years ago—were founded on sound principles amidst the great populations of Lancashire and Yorkshire, the university system of the country would gradually receive a large and useful extension, and, without losing any of its present valuable characteristics, would become more intimately related than hitherto with those great industries upon which mainly depend the strength and wealth of the nation.

If Great Britain is to retain the commanding position she has so long occupied in skilled manufacture, the highest training which can be brought to bear on practical science will, Dr. Andrews said, be imperatively required; and it would be a fatal policy if that training had to be sought for in foreign lands, because it could not be obtained at home. The country which depended unduly on the stranger for the education of its skilled men, or neglected in its highest places this primary duty, might expect to find the demand for such skill gradually to pass away, and along with it the industry for which it was wanted. That education in its highest sense, based on a broad scientific foundation, and leading to the application of science to practical purposes—in itself one of the noblest pursuits of the human mind—could be most effectively given in a university, or in an institution like the Polytechnic School of Zürich, which differed from the scientific side of a university only in name, and to a large extent supplemented the teaching of an actual university, he was firmly convinced; and for this reason, among others, he had always deemed the establishment in this country of examining boards with the power of granting degrees, but with none of the higher and more important functions of a university, to have been a measure of questionable utility. It was to Oxford and Cambridge, widely extended as they could readily be, that the country should chiefly look for the development of practical science; they had abundant resources for the task; and if they wished to secure and strengthen their lofty position, they could do it in no way so effectually as by showing that in a green old age they preserved the vigour and elasticity of youth.

Dr. Andrews instanced the University of Berlin. It was founded in the year 1810, at a period when the pressure of foreign domination weighed almost insupportably on Prussia; and Dr. Hofmann had remarked that it would ever remain significant of the direction of the German mind that the great men of that time should have hoped to develop, by high intellectual training, the forces necessary for the regeneration of their country," and in his recent report on the artificial dyes, M. Wurtz said—" Let us not suppose that the distance is so great between theory and its industrial applications. This report would have been written in vain if it had not brought clearly into view the immense influence of pure science upon the progress of industry. If unfortunately the sacred flame of science should burn dimly or be extinguished, the practical arts would soon fall into rapid delay. The outlay which is incurred by any country for the promotion of science and of high instruction will yield a certain return; and Germany has not had long to wait for the ingathering of the fruits of her far-sighted policy. Thirty or forty years ago, industry could scarcely be said to exist there; it is now widely spread and successful."

Dr. Andrews concluded by saying that " Whatever be the result of our efforts to advance science and industry, it requires no gift of prophecy to declare that the boundless resources which the supreme author and upholder of the universe has provided for the use of man will, as time rolls on, be more and more fully applied to the improvement of the physical and, through the improvement of the physical, to the elevation of the moral condition of the human family. Unless, however, the history of the future of our race be wholly at variance with the history of the past, the progress of

mankind will be marked by alternate periods of activity and repose; nor will it be the work of any one nation or of any one race. To the erection of the edifice of civilised life, as it now exists, all the higher races of the world have contributed; and if the balance were accurately struck, the claims of Asia for her portion of the work would be immense, and those of Northern Africa not insignificant. Steam power has of late years produced greater changes than probably ever occurred before in so short a time. But the resources of Nature are not confined to steam, nor to the combustion of coal. The steady water-wheel and the rapid turbine are more perfect machines than the stationary steam-engine; and glacier-fed rivers with natural reservoirs, if fully turned to account, would supply an unlimited and nearly constant source of power depending solely for its continuance upon solar heat. But no immediate dislocation of industry is to be feared, although the turbine is already at work on the Rhine and the Rhone. In the struggle to maintain their high position in science and its applications, the countrymen of Newton and Watt will have no ground for alarm so long as they hold fast to their old traditions, and remember that the greatest nations have fallen when they relaxed in those habits of intelligent and steady industry upon which all permanent success depends."

Two Evening Lectures were delivered; one by Prof. Tait, "On Force," and one by Sir Wyville Thomson, "On some Results of the *Challenger* Expedition." In his lecture "On Force" Prof. Tait observed that some criticisms on works in which he had at least had a share had shown him that even among the particularly well-educated class who wrote for the higher literary and scientific journals, there was widespread ignorance as to some of the most important elementary principles of physics. He had therefore chosen as the subject of a lecture a very elementary but much abused and misunderstood term, which met us at every turn in the study of natural philosophy. If one had a right to judge of the general standard of popular scientific knowledge from the statements made in the average newspaper, or even from those made in some of the most pretentious among so-called scientific lectures, there could be but few people in this country who had an accurate knowledge of the proper scientific meaning of the little word "force." We read constantly of the so-called "Physical Forces"—heat, light, electricity, &c.; of the "Correlation of the Physical Forces;" of the "Persistence or Conservation of Force." To an accurate man of science all this was simply error and confusion, and he had full confidence that the inherent vitality of truth would render the attempt to force such confusion upon the non-scientific public quite as futile as the hopelessly ludicrous endeavour of the "Times" to make us spell the word 'Chemistry" with a "y" instead of an "e." There was no objection to such phrases as "the force of habit," "the force of example," &c.; but when they read, as he had, in one newspaper, that the "force" of a projectile from the 81-ton gun had at last reached the extraordinary amount of 1450 feet, in another that the "force" of a ball from the great Armstrong gun lately made for the Italian Government was expected to average somewhere about 30,000 foot-tons, and in a third that the water in the boiler of the *Thunderer* "would in a second of time generate force sufficient to raise 2000 tons 1 foot high," they saw that there must be somewhere at least, if not everywhere, a most reckless abuse of lauguage. In fact they had come to what ought to be scientific statements, and there even the slightest unnecessary vagueness was altogether intolerable. Perhaps no scientific English word had been so much abused as the word "force." We hear of "Accelerating Force," "Moving Force," "Centrifugal Force," "Living Force," "Projectile Force," "Centripetal Force," and what not. Yet there was but one idea denoted by the word, and all force was of one kind, whether it was due to gravity, magnetism, or electricity. This alone served to give a preliminary hint that there was probably no such thing as force at all, but that it was merely a convenient expression for a certain "rate." Much of the confusion about Force was due to Leibnitz and some of his associates and followers, who, whatever they may have been

as mathematicians, were certainly grossly ignorant of some elementary parts of dynamics, insomuch that Leibnitz himself was known to have considered the fundamental system of the "Principia" to be erroneous, and to have devised another and different system of his own. This fact was carefully kept back now-a-days, but it was a fact, and it had a great deal to do with the vagueness of the terms for *Force* and *Energy* in some modern languages. In fact, in their modern dress, the *Vis Viva*, *Vis Mortua*, and *Vis Acceleratrix* of that time had, in some of their Protean shapes, hooked themselves, like Entozoa, into the great majority of our text-books.

The lecturer then proceeded to consider our modes of becoming acquainted with the physical world. In dealing with physical science it was absolutely necessary to keep well in view the all-important principle that—

"*Nothing can be learned as to the physical world save by observation and experiment, or by mathematical deductions from data so obtained.*"

The notion of force was suggested to us by the so-called muscular sense, which gives us a peculiar feeling of pressure when we attempt to move a piece of matter. The sense in which Newton used the word "force," and therefore the sense in which we must continue to use it if we desired to avoid intellectual confusion, would appear clearly from a brief consideration of his simple statement of the laws of motion. The first of these laws was—

"*Every body continues in its state of rest or of uniform motion in a straight line, except in so far as it is compelled by impressed forces to change that state.*"

In other words, any change, whether in the direction or in the rate of motion of a body, was attributed to force. Thus a stone let fall moved quicker and quicker, and we said that a force (viz., the weight of the stone, or the earth's attraction for it) was continually acting so as to increase the rate of the motion. If the stone were thrown upwards the rate of its motion continually diminished, and we said that the same force (the stone's weight) was continually acting so as to produce this diminution of speed. But this gave only half of the information which Newton's first law afforded. The moon revolved about the earth, and the earth and other planets revolved about the sun—approximately, at least, in circles. Why was this? Their directions of motion were constantly changing; in fact, a curved line was merely a line whose direction changed from point to point, while a straight line was one whose direction did not change; but to produce this change of direction force was required just as much as to produce change of speed. That was supplied by the gravitation attraction of the central body of the system. The old notion was that a centripetal force was required to balance the so-called centrifugal force, it being imagined that a body moving in a circle had a tendency to fly outwards from the centre! Newton's simple law exposed the absurdity of this. If a body was to be made to move in a curved line instead of its natural straight path, we must apply force to compel it to do so—certainly not to prevent it from flying outwards from the centre, about which it was for the moment revolving. In fact inertia meant not revolutionary activity, but dogged perseverance, and just as we must apply force in the direction of motion to change the rate of motion, so we must apply force perpendicular to the direction of motion to change that direction. Newton's second law was now required:—

"*Change of motion is proportional to the impressed force, and takes place in the direction of the straight line in which the force acts.*"

This one simple law held for all kinds of force alike. Change of motion was change of momentum, or the product of the mass of the moving body into its change of velocity. Of course the longer a given force acted the greater would be the change of momentum which it produced; so that to compare forces, which was the essence of the process of measuring them, we must give them equal times to act,—or, in scientific language, we must measure a force by the rate at which it produced change of momentum. Rate of change of velocity was called, in kinematics, acceleration. Thus the measure of a force was the product of the mass of the body moved into the acceleration which the force produced in it. This was the so-called *Vis motrix*, or "moving force" of the Cambridge text-books:—the so-called *Vis acceleratrix*, or "accelerating force," being really no force at all, but another name for the

kinematical quantity acceleration which he had just defined. Thus unit force was that force which, whatever its source, produced unit momentum in unit of time. If we employed British units—unit of force was that which, in one second, gave to one pound of matter a velocity of one foot per second. A pound of matter was a certain mass or quantity of matter. The weight of a pound of matter varied from place to place on the earth's surface—it depended on the attracting as well as the attracted body. The mass of a body was its own property. The earth's attraction for a body, or the weight of the body, was a force which produced in it in one second, a velocity which (in this latitude, and at the sea-level) was about 32·2 feet per second. Some people were in the habit of confounding force with momentum, but no one having sound ideas of even elementary mathematics could be guilty of this or any similar monstrosity. But to show to a non-mathematician that it was really monstrous to confound force and momentum, it was sufficient to change the system of units employed in measuring them, when it would be found that, if numerically equal for any one system of units, they were necessarily rendered unequal by a mere change of the unit employed for time. Now two things which were really equal to one another must necessarily be expressed by the same numerical quantity whatever system of units was adopted. Unit momentum was that of one pound of matter moving with a velocity of one foot per second. Unit force was that force which, acting for one second, produced in unit of mass a velocity of one foot per second. In each of these statements we might put an ounce or a ton, instead of a pound, and an inch or a mile in place of a foot, and their relative value would not be altered. But if we took a minute instead of a second as the unit of time, one foot per second was 60 feet per minute—so this change of the time unit increased sixty-fold the nominal value of the momentum considered. But in the case of the force our statement would stand thus:—What we formerly called unit of force was that which, acting for one-sixtieth only of our new unit of time produces in a mass of one pound sixty-fold the new unit of velocity. In other words, the number expressing the momentum was increased sixty-fold, while that representing the force was increased three thousand six hundred fold. In fact, whatever system of units we employed—if we increased in any proportion the unit of time, the measure of a momentum was increased in that proportion simply, while that of a force was increased in the duplicate ratio. The two things were, therefore, of quite dissimilar nature, and could not lawfully be equated to one another under any circumstances whatever. The mathematician expressed this distinction at once by saying that momentum was the time-integral of force, because force was the rate of change of momentum. Prof. Tait proceeded to say that the meaning of Newton's two first laws left absolutely no doubt as to the only definite and correct meaning of the word force. It was obviously to be applied to any pull, push, pressure, tension, attraction, or repulsion, &c., whether applied by a stick or a string, a chain or a girder; or by means of an invisible medium such as that whose existence was made certain by the phenomena of light and radiant heat, and which had been shown with great probability to be capable of explaining the phenomena of electricity and magnetism. There was then no such thing as centrifugal force; and accelerating force was not a physical idea at all. But that which was denoted by the term living force, though it had absolutely no right to be called force, was something as real as matter itself. To understand its nature we must have recourse to Newton's third law of motion, which was to the effect that—

"*To every action there is always an equal and contrary reaction; or, the mutual actions of any two bodies are always equal and oppositely directed.*"

This law Newton first showed to hold for ordinary pressures, tensions, attractions, impacts, &c. And when he said—"If any one presses a stone with his finger his finger is pressed with an equal and opposite force by the stone," we begin to suspect that force was a mere name—a convenient abstraction—not an objective reality. If we pulled one end of a long rope, the other being fixed, we could produce a practically infinite amount of force,

for there was stress across every section throughout the whole length of the rope. If we pressed upon a movable piston in the side of a vessel full of fluid we produced a practically infinite amount of force—for across every ideal section of the liquid a pressure per square inch was produced equal to that which we applied to the piston. If we let go the rope, or ceased to press on the piston, all this practically infinite amount of force was gone! But Newton proceeded to point out that this third law was true in another and much higher sense. He said:—

"*If the action of an agent be measured by the product of its force into its velocity; and if, similarly, the reaction of the resistance be measured by the velocities of its several parts into their forces, whether these arise from friction, cohesion, weight, or acceleration, action and reaction, in all combinations of machines, will be equal and opposite.*"

The actions and reactions which were here stated to be equal and opposite, were no longer simple forces, but the products of forces into their velocities; *i.e.*, they are what were now called rates of doing work; the time-rate of increase, or the increase per second of a very tangible and real someth ng, for the measurement of which rate Watt introduced the practical unit of a horse-power, or the rate at which an agent worked when it lifted 33,000 pounds 1 foot high per minute against the earth's attraction. With a moderate exertion we can raise a hundredweight a few feet, and in its descent it might be employed to drive machinery, or to do some other species of work. But tug as we pleased at a ton, we could not lift it; and therefore, after all our exertion, it would not be capable of doing any work by descending again. Thus it appears that force was a mere name, and that the product of a force into the displacement of its point of application had an objective existence. In fact, modern science showed us that force was merely a convenient term employed for the present to shorten what would otherwise be cumbrous expressions; but it was not to be regarded as a thing, any more than the bank rate of interest, be it 2, 2½, or 3 per cent., is to be looked upon as a sum of money, or than the birth-rate of a country is to be looked upon as the actual group of children born in a year. In fact, a simple mathematical operation showed us that it was precisely the same thing to say:—

"*The horse-power or amount of work done by an agent in each second is the product of the force into the average velocity of the agent,*"

and to say—

"*Force is the rate at which an agent does work per unit of length.*"

Following a hint given by Young, we now employed the term energy to signify the power of doing work, in whatever that power might consist. The raised mass, then, possessed, in virtue of its elevation, an amount of energy precisely equal to the work spent in raising it. This dormant, or passive, form was called potential energy. Excellent instances of potential energy were supplied by water at a high level, in virtue of which it could in its descent drive machinery—by the wound-up "weights" of a clock, which in their descent kept it going for a week; by gunpowder, the chemical affinities of whose constitutes were called into play by a spark, &c., &c. Another example of it was suggested by the word "cohesion," employed in Newton's statement, and which must be taken to include what are called molecular forces in general, such as, for instance, those upon which the elasticity of a solid depends. When we drew a bow, we did work, because the force exerted had a velocity; but the drawn bow (like the raised weight) had in potential energy the equivalent of the work so spent. That could in turn be expended upon the arrow. Now Newton spoke of one of the forms of resistance as arising from "acceleration." In fact the arrow, by its inertia, resisted being set in motion; work had to be spent in propelling it, but the moving arrow had that work in store in virtue of its motion. It appeared from Newton's previous statements that the measure of the rate at which work was spent in producing acceleration was the product of the momentum into the acceleration in the direction of motion, and the energy produced was measured by half the product of the mass into the square of the vivacity produced in it. This active form was called kinetic energy, and it was the double of this to which the term *vis viva*,

or living force, had been erroneously applied. As instances of ordinary kinetic energy, or of mixed kinetic and potential energies, we might take the following:—A current of water capable of driving an undershot wheel; winds, which also were used for driving machinery; the energy of water-waves or of sound waves; the radiant energy which comes to us from the sun, whether it affected our nerves of touch or of sight (and therefore be called radiant heat or light) or produced chemical decomposition, as of carbonic acid and water in the leaves of plants, or of silver salts in photography (and be therefore called actinism); the energy of motion of the particles of a gas, upon which its pressure depends, &c. When the motion is vibratory the energy is generally half potential, half kinetic. These explanations and definitions being premised, we could translate Newton's words into the language of modern science, as follows:—

"*Work done on any system of bodies* (in Newton's statement the parts of any machine) *has its equivalent in work done against friction, molecular forces, or gravity, if there be no acceleration; but if there be acceleration, part of the work is expended in overcoming the resistance to acceleration, and the additional kinetic energy developed is equivalent to the work so spent.*"

But we had just seen that when work was spent against molecular forces, as in drawing a bow or winding up a spring, it was stored up as potential energy. Also it was stored up in a similar form when done against gravity, as in raising a weight. Hence it appeared that, according to Newton, whenever work is spent it is stored up either as potential or as kinetic energy, except, possibly, in the case of work done against friction, about whose fate he gave us no information. Thus Newton expressly told us that, except, possibly, when there is friction, work is indestructible, it is changed from one form of energy to another, and so on, but never altered in quantity. To make this beautiful statement complete, all that was requisite was to know what became of work spent against friction. Here experiment was requisite. Newton, unfortunately, seemed to have forgotten that savage men had long since been in the habit of making it whenever they wished to procure fire. The patient rubbing of two dry sticks together, or the drilling of a soft piece of wood with the slightly blunted point of a hard piece, was known to all tribes of savages as a means of setting both pieces of wood on fire. Here, then, heat was undoubtedly produced, but it was produced by the expenditure of work. In fact work done against friction had its equivalent in the heat produced. This Newton failed to see, and thus his grand generalisation was left, though on one point only, incomplete. The converse transformation, that of heat into work, dated back to the time of Hero at least. But the knowledge that a certain process would produce a certain result did not necessarily imply even a notion of the "why;" and Hero as little imagined that in his æolipile heat was converted into work, as did savages that work could be converted into heat. But whenever any such conversion or transference took place there was necessarily motion: and the mere rate of conversion or transference of energy per unit length of that motion was in the present state of science conveniently called force. No confusion could arise from using such a word in such a sense. Rumford and Davy showed conclusively that the materiality of heat could not be maintained, and thus gave the means of completing Newton's statement which, still farther extended and generalised by Colding and Joule, was now known as the law of the conservation of energy. The conception of kinetic energy was a very simple one, when visible motion alone was involved. And from motion of visible masses to those motions of the particles of bodies whose energy we call heat, was by no means a difficult mental transition. Heat and kinetic energy in general were no more "modes of motion" than potential energy of every kind, including that of unfired gunpowder, was a "mode of rest!" In fact a "mode of motion" was, if the word motion be used in its ordinary sense, purely kinematical, not physical; and if motion were used in Newton's sense, it referred to momentum, not to energy. The conception of potential energy, however, was not by any means so easy or direct. In fact, the apparently direct testimony of our muscular sense to the existence of force made it at first much easier for us to conceive of force than of pote

tial energy. Why two masses of matter possess potential energy when separated, in virtue of which they are conveniently said to attract one another, was still one of the most obscure problems in physics. If the ingenious idea of the ultramundane corpuscles, the outcome of the life-work of Le Sage, and the only even apparently hopeful attempt which has yet been made to explain the mechanism of gravitation was true, it would probably lead us to regard all kinds of energy as ultimately kinetic. A singular quasi-metaphysical argument might be raised on this point, of which he could give only the barest outline. The mutual convertibility of kinetic and potential energy showed that relations of equality, though not necessarily of identity, could exist between the two, and thus that their proper expressions involved the same fundamental units, and in the same way. Thus, as we had already seen, that kinetic energy involved the unit of mass and the square of the linear unit directly, together with the square of the time-unit inversely, the same must be the case with potential energy; and it seemed very singular that potential energy should thus essentially involve the unit of time if it did not ultimately depend in some way on energy of motion. In defence of accuracy, which was the *sine quâ non* of all science, we must be "zealous," as it were, even to "slaying." And, as all the power of the *Times* would not compel us to put a *y* instead of an *e* into the word chemist, so neither would the bad example of Germany and France, though recommended to us with all the authority which might be attributed to an ex-president of the Association, succeed in inducing us to attach two or more perfectly distinct and incompatible scientific meanings to that useful little word, "force," which Newton had once and for ever defined for us with his transcendent clearness of conception.

The usual Lecture to Working Men was delivered by Lieut. Cameron, R.N., C.B., on his "Recent Journey across Africa."

The Mathematical and Physical Section was presided over by Professor Sir William Thomson, F.R.S. His opening address was mainly devoted to a review of evidence regarding the Physical Condition of the Earth; its Internal Temperature; the Fluidity or Solidity of its Interior Substance; the Rigidity, Elasticity, Plasticity of its External Figure; and the Permanence or Variability of its Period and Axis of Rotation. He first, however, referred to his recent visit to America. In the United States Government part of the Great Exhibition of Philadelphia, Prof. Hilgard showed him the measuring rods of the United States Coast Survey with their beautiful mechanical appliances for end measurement, by which the three great baselines of Maine, Long Island, and Georgia were measured with about the same accuracy as the most accurate scientific measures whether of Europe or America have attained in comparing two metre or yard measures. In the United States telegraphic department he saw and heard Elisha Gray's splendidly worked-out electric telephone actually sounding four messages simultaneously on the Morse code, and clearly capable of doing yet four times as many with very moderate improvements of detail; he also saw Edison's automatic telegraph delivering 1,015 words in 57 seconds; this done by the long-neglected electro-chemical method of Bain, long ago condemned in England to the helot work of recording from a relay, and then turned adrift as needlessly delicate for that. In the Canadian department he heard "To be or not to be—there's the rub," through an electric wire; but, scorning monosyllables, the electric articulation rose to higher flights, and gave him passages taken at random from the New York newspapers with unmistakable distinctness by the thin circular disc armature of a small electro-magnet. The words were shouted with a clear and loud voice by Prof. Watson at the far end of the line, holding his mouth close to a stretched membrane, carrying a little piece of soft iron, which was thus made to perform in the neighbourhood of an electro-magnet in circuit with the line motions proportional to the sonorific motions of the air. This, the greatest by far of all the marvels of the electric telegraph, was due to a young countryman of our own, Mr. Graham Bell, of Edinburgh and Montreal, and Boston. Who could but admire the hardihood of invention which devised such very slight means

to realise the mathematical conception that, "if electricity is to convey all the delicacies of quality which distinguish articulate speech, the strength of its current must vary continuously and as nearly as may be in simple proportion to the velocity of a particle of air engaged in constituting the sound?"

The Patent Museum of Washington, an institution of which the nation was justly proud, and the beneficent working of the United States patent laws, deserved notice. He was much struck with the prevalence of patented inventions in the Philadelphia Exhibition. He asked one inventor of a very good invention "why don't you patent it in England?" The answer was "The conditions in England are too onerous." We certainly were far behind America's wisdom in this respect. If Europe did not amend its patent laws (England in the opposite direction to that proposed in the bills before the last two sessions of Parliament) America would speedily become the nursery of useful inventions for the world. He might also mention "Old Prob's" weather warnings, which cost the nation 250,000 dollars a year; and though Democrats or Republicans playing the "economical ticket" might for half a session stop the appropriations for even the United States Coast Survey, no one would for a moment think of starving "Old Prob." The United States' Naval Observatory was full of the very highest science under the command of Admiral Davis. If to get on to precession and nutation, he had resolved to omit saying that he had there, in an instrument for measuring photographs of the Transit of Venus seen, for the first time in an astronomical instrument, a geometrical slide, the verdict on the disaster on board the *Thunderer*, published while writing his address, forbade him to keep any such resolution, and compelled him to put the question, "Is there in the British Navy, or in a British steamer, or in a British land boiler another safety-valve so constructed that by any possibility, at any temperature, or under any stress it can jam?" and to say that if there was it must be instantly corrected or removed.

Passing to the subject of his address, Sir William Thomson said that the evidence of a high internal temperature was too well known to need any quotation of particulars at present. Below the uppermost ten metres stratum of rock or soil sensibly affected by diurnal and annual variations of temperature, there was generally found a gradual increase of temperature downwards, approximating roughly, in ordinary localities, to an average rate of 1 deg. C. per thirty metres of descent, but much greater in the neighbourhood of active volcanoes, and certain other special localities of comparatively small area, where hot springs and perhaps also sulphurous vapours prove an intimate relationship to volcanic quality. It was worthy of remark in passing that so far as we know at present there were no localities of exceptionally small rate of augmentation of underground temperature, and none where temperature diminishes at any time through any considerable depth downwards below the stratum sensibly influenced by summer heat and winter cold. By a simple effort of geological calculus it had been estimated that 1 deg. per 30 metres gives 1000 deg. per 30 kilometres, and 3333 deg. per 100 kilometres. This arithmetical result was irrefragable, but what of the physical conclusion drawn from it with marvellous frequency and pertinacity that at depths of from 30 to 100 kilometres the temperatures are so high as to melt all substances composing the earth's upper crust? It had been remarked, indeed, that "if observation showed any diminution or augmentation of the rate of increase of underground temperature in great depths, it would not be right to reckon on the uniform rate of 1 deg. per 30 metres, or thereabouts, down to 30, or 60, or 100 kilometres. But observation has shown nothing of the kind, and, therefore, surely it is most consonant with inductive philosophy to admit no great deviation in any part of the earth's solid crust from the rate of increase proved by observation as far as the greatest depths to which we have reached." Now he had to remark upon this that the greatest depths to which we have reached in observations of underground temperature was scarcely one kilometre; and that if any falling offof the rate of augmentation of underground temperature was sensible at a depth of one kilometre, this would demonstrate that within the last 10,000 years the upper surface of the earth must have been at a higher temperature than that now found at the depth of one kilometre.

Such a result was, no doubt, to be found by observation in places which had been overflown by lava in the memory of man or a few years further back; but if it were found for the whole earth, it would limit the whole of geological history to within 10,000 years, or, at all events, would interpose an absolute barrier against the continuous descent of life on the earth from earlier periods than 100,000 years ago. Therefore, although search in particular localities for a diminution of the rate of augmentation of underground temperature in depths of less than a kilometre might be of intense interest, as helping us to fix the dates of extinct volcanic actions which had taken place within 10,000 years or so, we know enough from thoroughly sure geological evidence not to expect to find it, except in particular localities, and to feel quite sure that we should not find it under any considerable portion of the earth's surface. If we admit as possible any such discontinuity within 900,000 years, we might be prepared to find a sensible diminution of the rate at three kilometres' depth, but not at anything less than 30 kilometres if geologists validly claim as much as 90,000,000 of years for the length of the time with which their science is concerned. Now, this implied a temperature of 100 deg. C. at the depth of 30 kilometres, allowed something less than 2000 deg. for the temperature at 60 kilometres, and did not require much more than 4000 deg. C. at any depth, however great, but did require at the great depths a temperature of, at all events, not less than about 4000 deg. C. It would not take much 'hurrying-up' of the actions with which they are concerned to satisfy geologists with the more moderate estimate of 50,000,000 of years. This would imply, at least, about 3000 deg. C. for the limiting temperature at great depths. If the actual substance of the earth, whatever it might be, rocky or metallic, at depths of from 60 to 100 kilometres, under the pressure actually there experienced by it, could be solid at temperatures of from 3000 deg. to 4000 deg., then we might hold the former estimate (90,000,000) to be as probable as the latter (50,000,000), so far as evidence from underground temperature could guide us; if 4000 deg. would melt the earth's substance at a depth of 100 kilometres, we must reject the former estima e, though we might still admit the latter; if 3000 deg. would melt the substance at a depth of 60 kilometres, we should be compelled to conclude that 50,000,000 of years was an over estimate. Whatever might be its age, we might be quite sure the earth was solid in its interior; not absolutely throughout its whole volume, for there certainly were spaces in volcanic regions occupied by liquid lava; but whatever portion of the whole mass was liquid, whether the waters of the ocean or melted matter in the interior, these portious were small in comparison with the whole; and we must utterly reject any geological hypothesis which, whether for explaining underground heat or ancient upheavals and subsidences of the solid crust, or earthquakes, or existing volcanoes, assumed the solid earth to be a shell of 30 or 100, or 500, or 1000 kilometres thickness, resting on an interior liquid mass. This conclusion was first arrived at by Hopkins, who might, therefore, properly be called the discoverer of the earth's solidity. He was led to it by a consideration of the phenonema of precession and nutation, and gave it as shown to be highly probable, if not absolutely demonstrated, by his confessedly imperfect and tentative deduction, but a rigorous application of the perfect hydrodynamical equations leads still more decidedly to the same conclusion. Sir William Thomson asked those who possessed the "Transactions of the Royal Society" for 1862, and of Thomson and Tait's "Natural Philosophy," Vol. 1, to draw the pen through sections 23-31 of his paper on the "Rigidity of the Earth," in the former, and through everything in sections 847-849 of the latter, which referred to the effect on precession and nutation of an elastic yielding of the earth's surface, for he had convinced himself that those conclusions at which he had arrived by a non-mathematical short cut were grievously wrong.

A little consideration sufficed to show him that a very slight deviation of the inner surface of the shell from perfect sphericity would suffice, in virtue of the quasi-rigidity due to vortex motion, to hold back the shell from taking sensibly more precession than it would give to the liquid, and to cause the liquid (homogeneous or heterogeneous) and the shell to have sensibly the

same precessional motion as if the whole constituted one rigid body. But although so much could be foreseen readily enough, he found it impossible to discover, without thorough mathematical investigation, what might be the characters and amounts of the deviations from a rigid body's motion which the several cases of precession and nutation contemplated would present. The investigation, limited to the case of a homogeneous liquid enclosed in an ellipsoidal shell, had brought out results which had greatly surprised him. When the interior ellipticity of the shell is just too small, or the periodic speed of the disturbance just too great to allow the motion of the whole to be sensibly that of a rigid body, the deviation first sensible renders the precessional or nutational motion of the shell smaller by a small difference than if the whole were rigid, instead of greater, as he expected. The amount of this difference bears the same proportion to the actual precession or nutation as the fraction measuring the periodic speed of the disturbance (in terms of the period of rotation as unity) bears to the fraction measuring the interior ellipticity of the shell; and it is remarkable that this result is independent of the thickness of the shell, assumed, however, to be small in proportion to the earth's radius. The conclusions to which Sir William's investigations led, he considered to be absolutely decisive against the geological hypothesis of a thin rigid shell full of liquid. But, he proceeded, interesting in a dynamical point of view as Hopkins's problem is, it could not afford a decisive argument against the earth's interior liquidity. It assumed the crust to be perfectly stiff and unyielding in its figure; but if it consisted of continuous steel and 500 kilometres thick, it would yield very nearly as much as if it were india-rubber, to the deforming influences of centrifugal force and of the sun's and moon's attractions. The state of the case was shortly this:—The hypothesis of a perfectly rigid crust containing liquid violates physics by assuming preternaturally rigid matter and dynamical astronomy in the solar semi-annual and lunar fortnightly nutations; but tidal theory has nothing to say against it. On the other hand the tide, decides against any crust flexible enough to perform the nutations correctly with a liquid interior, or as flexible as the crust must be unless of preternaturally rigid matter.

But thrice to slay the slain; suppose the earth this moment to be a thin crust of rock or metal resting on liquid matter. Its equilibrium would be unstable. The upheavals and subsidences would be strikingly analogous to those of a ship which had been rammed; one portion of crust up and another down, and then all down. Whatever might be the relative densities of rock solid and melted, at or about the temperature of liquefaction, it was, he thought, quite certain that cold solid rock was denser than hot melted rock; and no possible degree of rigidity in the crust could prevent it from breaking in pieces and sinking wholly below the liquid lava. Something like this probably went on for thousands of years after solidification commenced; surface portions of the melted material losing heat, freezing, sinking immediately, or growing to thickness of a few metres when the surface would be cool and the whole solid dense enough to sink. "This process must go on until the sunk portions of crust build up from the bottom a sufficiently close-ribbed skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava pools or lakes. In the honey-combed solid and liquid mass thus formed there must be a continual tendency for the liquid, in consequence of its less specific gravity, to work its way up, whether by masses of solid falling from the roofs of vesicles or tunnels, and causing earthquake shocks, or by the roof breaking quite through when very thin, so as to cause two such hollows to unite or the liquid of any of them to flow out freely over the outer surface of the earth; or by gradual subsidence of the solid owing to thermodynamic melting, which portions of it under intense stress must experience according to his brother's theory. The results which must follow from this tendency seem sufficiently great and various to account for all that we learn from geological investigation of earthquakes, of upheavals, and subsidences of solid, and of eruptions of melted rock."

Leaving altogether now the hypothesis of a hollow shell filled with liquid, we must still face the question, how much does the earth, solid throughout,

except small cavities or vesicles filled with liquid, yield to the deforming (or tide generating) influences of sun and moon? This question could only be answered by observation. A single infinitely accurate spirit level or plummet far enough away from the sea to be not sensibly affected by the attraction of the rising and falling water, would enable us to find the answer. Closely connected with the question of the earth's rigidity, and of as great scientific interest and even greater practical moment, was the question—how nearly accurate is the earth as a timekeeper? and another of, at all events, equal scientific interest—how about the permanence of the earth's axis of rotation? Peters and Maxwell, about thirty-five and twenty-five years ago, separately raised the question, how much does the earth's axis of rotation deviate from being a principal axis of inertia? and pointed out that an answer to this question was to be obtained by looking for a variation in latitude of any or every place on the earth's surface in a period of 306 days. Peters gave a minute investigation of observations at Pulkova in the years of 1841-42, which seemed to indicate at that time a deviation amounting to equal to about 3-40 seconds of the axis of rotation from the principal axis. Maxwell from Greenwich found seeming indications of a very slight deviation—something less than half a second—but differing altogether in phase from that which the deviation indicated by Peters, if real and permanent, would have produced at Maxwell's later time. On his (Sir William Thomson) begging Prof. Newcomb to take up the subject, he undertook to analyse a series of observations suitable for the purpose, which had been made in the United States Naval Observatory, Washington. A few weeks later he received from him a letter referring him to a paper by Dr. Nysen, of Pulkova Observatory, in which a negative conclusion as to constancy of magnitude or direction in the deviation sought for is arrived at from several series of the Pulkova observations between the years 1842 and 1872, and containing a statement of his own conclusions, which were also negative. From the discordant character of these results we must not, however, infer that the deviations indicated by Peters, Maxwell, and Newcomb were unreal. On the contrary, any that fall within the limits of probable error of the observations ought properly to be regarded as real. There was in fact a *vera causa* in the temporary changes of sea level due to meteorological causes, chiefly winds, and to meltings of ice in the polar regions, and return evaporations, which seemed amply sufficient to account for irregular deviations of from one-half to 1-20th second of the earth's instantaneous axis from the axis of maximum inertia, or, as he ought rather to say, of the axis of maximum inertia from the instantaneous axis." Sir William concluded his address by considering the variations in the earth's rotational period.

The report of the Committee for Testing Experimentally the Exactness of Ohm's Law, drawn up by Prof. Clerk Maxwell, was read by Mr. Chrystal. The result of this investigation is described as follows:—If a conductor of iron, platinum, or German silver of one square centimetre in section has a resistance of one ohm for infinitely small currents, its resistance when acted on by an electromotive force of one volt (provided its temperature is kept the same) is not altered by so much as the millionth of a millionth part. The report concludes:—" It is seldom, if ever, that so searching a test has been applied to a law which was originally established by experiment, and which must still be considered a purely empirical law, as it has not hitherto been deduced from the fundamental principles of dynamics. But the mode in which it has borne this test not only warrants our entire reliance on its accuracy within the limits of ordinary experimental work, but encourages us to believe that the simplicity of an empirical law may sometimes be an argument for its exactness, even when we are not able to show that the law is a consequence of elementary dynamical principles."

One of the most important papers contributed to the section was that by Professor Stokes on "The Phenomena of Metallic Reflection." Professor Stokes explained that when Newton's rings were formed between a lens and a

plate of metal, and were viewed by light polarised perpendicularly to the plane of incidence, it was known that, as the angle of incidence was increased, the rings which were at first dark-centred, disappeared in passing the polarising angle of the glass, and then reappeared white-centred, in which state they remained up to a growing incidence, when they could no longer be followed. At a high incidence the first dark ring was much the most conspicuous. To follow the rings beyond the limit of total internal reflection, a prism must be employed. When the rings formed between glass and glass were viewed in this way, as the angle of incidence was increased the rings one by one opened out, uniting with bands of the same respective orders which were seen beneath the limit of total internal reflection; the limit or boundary between total and partial reflection passed down beneath the point of contact, and the central dark spot was left isolated in a bright field. Now, when the rings were formed between a prism with a slightly convex base and a plate of silver, and the angle of incidence was increased so as to pass the critical angle, if common light be used in lieu of a simple spot, we had a ring which became more conspicuous at a certain angle of incidence well beyond the critical angle, after which it rapidly contracted and passed into a spot. To study the phenomenon in its purity it is necessary to employ polarised light, or, which is more convenient, analyse the reflected light by means of a prism. This phenomenon was discovered by Professor Stokes many years ago, but he has only just completed the necessary investigations.

Dr. Ker, to whose valuable discovery of the double refracting property produced in a dielectric by electric induction we referred in a recent number of this journal, described "An Experiment proving Rotation of the Plane of Polarisation of Light Reflected from a Magnetic Pole." If plane polarised light be allowed to fall on the polished extremity of the soft iron core of an electro-magnet, and a Nicol's prism be fixed in such a position as to extinguish the reflected light, the current being not yet sent through the coil of the magnet, on sending the current, the light is restored. In order to increase the magnetisation of the reflecting surface, or that portion of it which is utilised, the wedge-shaped termination of a mass of iron is held close over it. Sir W. Thomson remarked that this experiment proved iron to possess, in an enormously higher degree, the property discovered by Faraday for heavy glass.

In a paper "On the Protection of Buildings from Lightning," Professor Clerk Maxwell proposed to take advantage of the fact that an electric discharge cannot occur between two bodies unless their difference of potential was sufficiently great compared with the distances between them, but it was shown by experiment that if every part of the surface surrounding a certain region is at the same potential, every point within that region must be at the same potential, provided no charged body is within the region. If a powder mill were coated on the roof, walls, and floor with sheet copper, if all conductors, such as water-pipes, entering the building were connected with the coating, no electrical effect could be produced in the interior: but in practice, even this was superfluous in this climate. For ordinary buildings it would suffice to have a copper wire carried round the foundation of the house, up each of the corners and gables, and along the ridges. The copper wires might be built into the walls to prevent theft. In the case of a powder-mill, it might be advisable to make the network closer by carrying wires over the walls. It was advisable not to erect a tall condactor with a sharp point in order to relieve the thunder clouds of their charge.

A paper "On the Influence of the Residual Gas on the Movement of the Radiometer" was read by Mr. Crookes. His recent experiments show that the movement of this instrument is not due to a direct repulsion exerted by light on the vanes, but to a mutual action called out between these vanes and the very attenuated gas remaining in the instrument. It is well known that, with a moderately good vacuum, the motion becomes more rapid as the exhaustion proceeds; but Mr. Crookes has recently succeeded in producing such a complete exhaustion that he not only reaches the point of maximum effect, but goes far beyond it, so far that the effect nearly ceases. He measures the

vacuum by means of a special apparatus, in which, instead of continuously rotating in one direction. as in the ordinary radiometer, the moving part is suspended by a glass fibre, which it twists in opposite directions alternately. The movement is started by rotating the whole apparatus through a small angle, and the observation consists in noting the successive amplitudes of vibration when the instrument is left to itself, a mirror and spot of light being employed for this purpose. The amplitudes form a decreasing series, with a regular logarithmic decrement. The logarithmic decrement is nearly constant up to the point at which the vacuum is apparently equal to a Torricellian vacuum, the mercury in the gauge standing at the same height as a barometric column beside it; but as the exhaustion proceeds beyond this point, the logarithmic decrement becomes smaller; in other words, the amplitude diminishes less rapidly. By plotting the observations and supposing the curve continued, it is indicated that, if a perfect vacuum were attained, the logarithmic decrement would be zero, we should have perpetual motion with constant amplitude, and at the same time, the radiometer would cease to act. He had tried other gases as well as air. Aqueous vapour is very unfavourable to the action of the radiometer; hydrogen, on the contrary, gave the best result of all. Prof. O. Reynolds and Mr. Schuster had published experiments which seemed to point to the true explanation of the action of the radiometer; but he thought Mr. Stoney's explanation the clearest. The molecules of the gas are beating about with the temperature; but, in tolerably dense air, they jostle one another, and their paths are short. When exhaustion is carried to a certain high point, the molecules are sufficiently few, and their paths sufficiently long for them to rebound from the glass.

We are compelled from want of space to omit notices of other valuable papers brought before Section A. These, with a report of the work done in the other sections, will be given in our next number.

Microscopy.—Mr. A. M. Edwards, of Newark, New Jersey, has experimented upon the properties of salicylic acid as a preservative for microscopical purposes. Casts of uriniferous tubules, obtained from a severe case of nephritis, mounted in 1874 in a dilute solution, are in as good condition as when first put up. Mr. Edwards has also succeeded with *Volvox globatur*. Salicylic acid is worth further experiment, as our preservative fluids are as yet but imperfectly understood, and researches upon their properties much needed.

Labarraque's solution, so frequently mentioned in various American methods of staining vegetable tissues, and used for bleaching preparatory to the dyeing process, is the "*Liquor Soda Chlorinata*" of the British Pharmacopœia.

Mr. William A. Rogers contributes to the "Proceedings of the American Academy of Arts and Sciences" a paper "On a Possible Explanation of the Method Employed by Nobert in Ruling his Test Plates." Mr. Rogers has himself succeeded in ruling plates as fine as 80,000 lines to the inch. The paper contains three tables relating to the errors in spacing in plates ruled by Rutherford, Nobert, and the author, and a somewhat elaborate account of the mechanical contrivances employed in moving the plate to be ruled over given and equal spaces. The comprehension of this portion would be greatly aided by suitable illustrations. The tool used for ruling by Mr. Rutherford and Mr. Rogers is a diamond having a knife edge; that used by Nobert is known only to himself. The stone is worked artificially to the required form, the use of the natural crystal, either whole or broken into chance fragments, having long ago been abandoned. In these experiments it has been discovered that glass has what may for want of a better term be described as a *grain*, fine, clean lines being only capable of being ruled in certain directions. It is a remarkable fact that a properly-shaped diamond, set in the best position, is not at first capable of ruling the finest lines, but improves with use. When the diamond does its work perfectly the cut, even of the finest line, produces a sharp, singing sound, so that the operator can judge of the quality of the lines ruled almost as well by hearing as by sight. The best results have,

however, been obtained with a black carbon worked to a knife edge: this stone is much harder than any other variety of diamond, the process of grinding to an edge occupying from five to ten days. Mr. Rogers's investigations have been the result of experiments undertaken for a different purpose. Failing to find a spider that would spin suitable web for the telescope of the meridian circle of Harvard College Observatory, an effort was made to produce upon glass lines of the desired quality and size. The object was accomplished, and the investigations have been continued during a period of about three years.

MINERALOGY.

Dr. F. A. Genth has recently published a paper "On Some American Vanadium Minerals." He first mentions Roscoelite, a vanadium mica found by Dr. Blake, of San Francisco, California, named and called by him "Roscoelite," in honour of Professor Roscoe, whose important investigations have put vanadlum in its proper place among the elements. Roscoelite occurs in small seams, varying in thickness from 1-20th to 1-10th of an inch in a decomposed yellowish, brownish, or greenish rock. These seams are made up of small micaceous scales, sometimes ¼ of an inch in length, mostly smaller and frequently arranged in stellate or fan-shaped groups. They show an eminent basal cleavage; soft; the sp. gr. of the purest scales (showing less than 1 per cent. of impurities) was found to be 2·938; another specimen of less purity gave 2·921; lustre pearly, inclining to submetallic; colour, dark clove-brown to greenish brown, sometimes dark brownish green. Before the blowpipe it fuses easily to a black glass, colouring the flame slightly pink. With salt of phosphorus gives a skeleton of silicic acid, a dark yellow bead on the oxidising flame, and emerald green bead in the reducing flame. Only slightly acted upon by acids, even by boiling concentrated sulphuric acid; but readily decomposed by dilute sulphuric acid, when heated in a sealed tube at a temperature of about 180° C., leaving the silicic acid in the form of white pearly scales, and yielding a deep bluish green solution. With sodic carbonate it fuses to a white mass.

A remarkable deposit of tripolite, existing in the island of Barbadoes, where it is mixed with a certain quantity of carbonate of lime, has been examined by Dr. T. L. Phipson. Under the microscope it is found to be exceedingly rich in remains of fossil Infusoria, the forms of which are very well preserved. The silica is hydrated and soluble to a great extent in potash solution, and, like tripolite from other localities, it constitutes an excellent polishing material. On account of its value in this respect tripolite has many imitations in commerce, but it can be recognised at once by analysis, and also by the microscope. Among other useful purposes to which the Barbadoes tripolite has been applied latterly, we may mention that, having been found a bad conductor of heat, it has been used with advantuge for covering boilers.

In a paper on "American Tellurium and Bismuth Minerals," read before the American Philosophical Society at the meeting of August 21, 1874, Dr. Genth mentioned, on the authority of Mr. P. Knabe, a siskin green pulverulent mineral from the "Iron Rod Mine," Silver Star District, Montana, as a *new* "Tellurate of lead and copper." At that time he nad no opportunity of examining into the merits of this mineral. A qualitative examination proved it to be a hydrous *vanadate* of lead and copper and *not a tellurate.*

The nickel ores of New Caledonia are found by M. J. Garnier to be not arsenio-sulphides like those hitherto utilised, but silicates of nickel and magnesia. The ore is found amidst the masses of serpentine very abundant in certain parts of the island, and associated with euphotides, diorites, amphibolites, &c. The nickel is accompanied by iron, chrome, and cobalt; these metals, especially the two former, are of an unexampled abundance. The cobalt is associated with manganese. These ores have also been examined by MM. P. Christofle and H. Bouilhet, who find that they belong to three distinct types—An emerald green hydrosilicate, compact and hard, containing 18 to 20 per cent of nickel and 5 per cent of water; a yellowish green hydro-

silicate, more friable, and containing 12 to 15 per cent of nickel and 10 to 15 of water; a whitish blue hydrosilicate, very brittle, and easily crushed with the fingers, containing merely 6 to 8 per cent of nickel, and as much as 20 per cent of water. The metallic nickel extracted from these ores contains from 98 to 99·5 per cent of pure nickel.

The Mineralogical Society of Great Britain and Ireland has issued the first number of its journal. A meeting of the society was held at Redruth on July 1st, when the secretary, Mr. J. H. Collins, read a paper on a new mineral from West Phœnix Mine which he has named "Henwoodite." It is a hydrous phosphate of alumina and copper, resembling turquoise, but containing more phosphoric acid and water and less alumina. Dr. C. Le Neve Foster read a paper on the occurrence of pyrophillite at Brookwood Mine, and on new mineral localities in Devon and Cornwall, in which he described a new mineral "Enysite," a hydrated compound of alumina and copper, with some sulphates, carbonates, and chlorides.

Mr. William Vivian contributed a note on paragenetic formations of carbonate of lime and oxide of iron, and of quartz, at the Mwyndy Mines, Glamorganshire. These specimens form interesting objects for the microscope, they are best seen as opaque objects in a good light and with a low power from 1″ to 2″ objectives. Mr. Collins remarked that these constant occurrences strengthen the evidence already existing that such iron ore deposits as that at Mwyndy, like those in the lodes or cross-courses of Cornwall and Devon, are due to infiltration of ferruginous matter from the surrounding rocks into pre-existing fissures.

At the same meeting Mr. Collins noted the occurrence of scorodite, pharmacosiderite, and olivenite in greenstones at Terras Mine, St. Stephens, and Dr. Foster exhibited a new form of blowpipe lamp suitable for travellers.

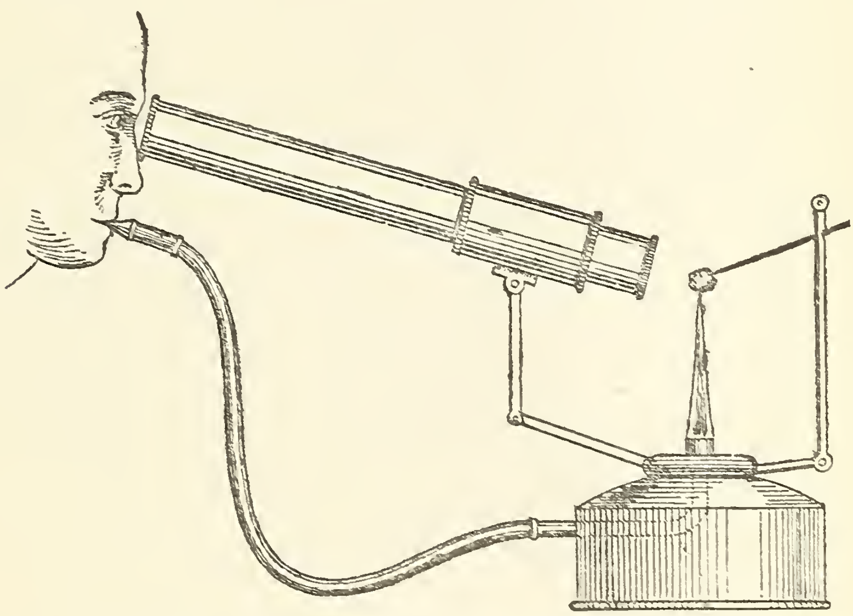
A meeting of the society was held in Glasgow on September 6th, when Professor Heddle delivered an address "On Scotch Minerals, how and where to find them."

Dr. Haughton, of Trinity College, Dublin, delivered an address on a new principle in the consolidation of porphyritic rocks. He received a collection of lava from Vesuvius, with the view of ascertaining if, in the space of 300 years, any difference had occurred in the composition of the lava flows. In the course of the investigation he ascertained that the minerals combined with a substance like paste, and his researches led to the conclusion that when a rock crystallises the maximum of minerals will form with a minimum of paste; this "principle of least paste" is confirmed by chemical experiments.

The Secretary read a paper by the president, Mr. H. C. Sorby, "On the Critical Point in the Consolidation of Granite." In the opinion of the author the critical point in the consolidation of granite is very closely connected with the critical temperature at which highly heated and compressed steam condenses into an equal volume of highly heated and expanded liquid water. Rocks melted under great pressure probably contain water either dissolved as a gas in a liquid or in the state of a fused hydrate. On cooling down to a lower temperature the crystallising out of minerals almost necessarily sets free the previously combined water. As long as the temperature is above the critical point it necessarily remains more or less disseminated in the rocks as a highly compressed steam, but as soon as the temperature falls below the critical point it condenses into highly expanded liquid water. The only determination of this temperature with which the author is acquainted is that of Cagmard Latur, who found that water expanded to nearly four times its volume, and passed into an equal volume of highly compressed steam, at about the melting point of zinc, or 412° C. As long as the temperature is higher than this the solvent action of water cannot be brought into play, since earthy substance and alkaline salts cannot be dissolved in steam; but as soon as it condenses into a liquid the solvent power of the water will be very great, as shown by its intense action on glass. These theoretical deductions agree remarkably well with the microscopical structure of the Pouza granite.

The Secretary read notes on a mineral from New South Wales, presumed to be lamontite, by Prof. Liversidge, F.G.S., "Some Notes from an Old Catalogue of Minerals," by Prof. A. H. Church, and "Notes on Occurrence of Achroite at the Rock Hill, in the parish of Austell, Cornwall, and on the Black Tourmaline of the same locality," by J. H. Collins.

The following is an illustration of an upright blowpipe with spectroscope adapted to its little lamp, contrived by Capt. Marshall Hall, for the purpose of discriminating in travelling between potass and soda and many other minerals. Capt. Hall remarks that "with hammer, chisel, lens, bottle of acid, magnetic penknife, and a little patience one can be far more independent of a laboratory than might be imagined. It is far more interesting to be able to determine a mineral on the spot, more especially as regards petrology, than to have to collect extensively and defer examination except with the blowpipe, which, as every one has to his aggravation experienced, leaves one sadly in the lurch when one gets amongst impure alkalies and alkaline earths. As an instance he has detected both baryta and strontia in arragonite, which blowpipe *solus* failed to show."



We have received "A Catalogue of the Published Works of Isaac Lee, LL.D., from 1817 to 1870," which contains an enumeration of 223 papers on mineralogy, geology, palæontology, and malacology.

GEOLOGY.

The second part of the eleventh volume of the "Memoirs of the Geological Survey of India," published under the direction of Dr. Thomas Oldham, F.R.S., is devoted to an account of the Trans-Indus salt region in the Kohat district. In this locality occur some of the largest exposures of rock salt known to exist upon the globe. The area occupied by these deposits is included within about a thousand square miles of country. The general aspect of the country is wild, rocky, and barren, almost bare of trees, and supporting nothing worthy of the name of jungle. The colouring of the ground is often vivid, whole ranges taking a green tint, not from grass but from the colour of the rocks, which are strongly contrasted with others of a bright purple. Zones of blood-red clays are varied by the white of adjacent gypsum, and interspersed with pale orange débris. The salt-beds occur in a nummulitic limestone, probably of the eocene period, and immediately below white, grey, and black gypsum with bands of dark grey clay, or black alum shale, sometimes impregnated with petroleum or bitumen. The upper part of the salt rock is bituminous, and its thickness varies from 300 to 1200 feet. In

many places the salt is largely exposed, forming high detached hills and cliffs. It has a whitish grey colour, and varies in texture from a highly crystalline mass, its most common form to an earthy condition interspersed with finely divided clay. Much of the salt is remarkably pure, and so far as is yet known, it is without a trace of associated salts of potassa. The Kohat salt is found to contain—Insoluble matter, 0·5; sulphate of lime, 2·5; magnesia, calcium, and sulphates, 0·0. On the other hand, the best Cis-Indus salt contains nearly 2 per cent of foreign salts. The strata underlying the salt are as yet unknown. The difference between the salt deposits of the two regions are so unlike that even small samples, if not pulverised, are declared capable of being sworn to with confidence by the officials whose task it is to prevent the importation of the Kohat salt into the country east of the Indus. The rock salt of Persia, far to the westward, has probably no close connection with that of Kohat, since this overlies the nummulitic formation. The salt of Ormuz in its occurrence amongst dolerites, trachytes, and micaceous iron presents also no analogy with that of the Kohat. No organic remains have been discovered iu the overlying gypsum, nor in the accompanying clays, except some obscure grass-like fragments. Some small impressions of shells are detected in a limestone band near the top of the gypseous formation. The resources of the district, in an economic point of view, are in addition to salt, gypsum, sulphur, alum, building stones, and coal. The latter mineral was first brought under notice by a native officer, and information concerning its quantity is as yet wanting. The total quantity of salt is estimated at 40 milliards of maunds, sufficient, after making am ample allowance for waste, to last, at the present rate of consumption, for 40,000 years.

In the eighth volume of the "Records of the Geological Survey of India," we find an interesting paper, by Mr. Fedden, on the former existence of ground ice in India. At Irai, in latitude 19° 53′, and at an elevation under 900 feet above the sea-level, the evidences of glacial action are considered as conclusive as those for the ice-age formation in Europe. There is also a notice of fire-bricks made from clay obtained at Mallapur. They were pronounced superior to Stourbridge bricks, but inferior to those of Glenboig. The Shapur coal-field, and the coal explorations in the Narbada region are described by Mr. H. B. Medlicott. The author thinks that there are good prospects in the Shapur district on the Tawa, if not in the Senada area. On the lower Narbada the hope of coal is somewhat more precarious. A sample from Naobelaka gave on analysis—Moisture, 3·4; volatile matter (not water), 39·6; fixed carbon, 55·2; ash, 1·8. Total, 100·0. Mr. Ball reports on the Raigarh and Hingir coal-fields. There are many seams of coal, that of Dibdorah being at least six and a half feet in thickness and of fair quality. Within the Barakar group there are two if not three zones of iron-stone, some of the ores appearing to be of good quality.

Volume nine, part 1, comprises the annual report of the Geological Survey of India for the past year. We may particularly notice Mr. Medlicott's labours iu the coal-fields of the Satpure hills, where deep borings are recommended to test the bulk of the coal deposits and their depth beneath the surface. Dr. W. Waagen has been obliged to relinquish his connection with the survey on account of his health. The remainder of this issue is devoted to the geology of Scinde, 9000 square miles of which province have been subjected to a preliminary survey. The importance of a thorough examination of Scinde is due to two circumstances: it has long been known that there exists in that province a fine series of tertiary rocks, rich in fossils. Further, the figures and descriptions of the Indian nummulitic fossils, published by D'Archiac and Haime in their "Description des Animaux fossiles du Groupe Nummulitique de l'Inde," lose much of their value, from the circumstance that the exact position in the series of the beds from which the different fossils described were obtained, is unknown. The majority of the fossils have been procured in Scinde, but the exact localities were not recorded. The investigation of this province is in the hands of Mr. W. T. Blanford, F.R.S., the deputy superintendent of the survey.

The "American Journal of Science and Arts" contains a paper by Mr. G. K. Gilbert on "The Colorado Plateau Region considered as a Field for Geological Study." This region is 170,000 square miles in extent, and is remarkable for the rocky character of its surface and its extreme aridity. The only mineral product of economic importance is coal, which is worked only where the Pacific Railroad affords means of transport. "The air is so dry that, except on the heights and on the margins of springs and streams, there is no turf, no accumulation of humus, often no soil, and so little vegetation that the view is not obstructed. From a commanding eminence one may see spread before him, like a chart, to be read almost without effort, the structure of many miles of country. There is no need to search for exposure where everything is exposed. The strata are undisturbed, and are cut by valleys of erosion, in the wall-like sides of which every inch of the series may be examined." It is evident that such a region must afford remarkable facilities for studying mountain-building by displacement, stratigraphy, and the problem of the canons. The author remarks—"Already the field has yielded to its students results new to them, and probably new to the world of science. Among them are a type of uplifted mountains, a type of eruptive mountains, a theory of water-falls, and a classification of drainage-systems."

Under the title of "The Geology of Portions of our Western Territory," Mr. Gilbert gives the results of an examination of portions of Nevada, Utah, California, and Arigona. He treats of the orology of the district, of its valleys and canons, of the glacial epoch, the water-supply, the volcanic rocks and mountains, and of the stratified rocks. Concerning the glaciation, he infers that the general glaciation of the Eastern States had no counterpart, in the same latitudes over the region extending from the Rocky Mountains to the Sierra Nevada inclusive. There were in that region local glaciers high upon the flanks of the mountains, the most southerly of which did not extend lower than an altitude of 8000 feet above the sea. There was a general accession of water to the valleys of the great basin. Lakes were formed where there are now only deserts, and valleys now nearly empty were filled to overflowing. The flooding of the valleys is correlated in time with the formation of glaciers upon the mountain summits on the same principle on which the different local floods are correlated with each other, the local glaciers with each other, the glaciers of the East with those of the West, and those of America with those of Europe,—namely, that the phenomena were of the same class and occurred in the same division of geological time. Each took place during the post-tertiary, and each marked a climatal change of polar tendencies. The phenomena of the Glacial epoch at the West differed from the synchronous phenomena in the same latitudes at the East, for the reason that then, as now, the former region was comparatively arid, and material was lacking for a great ice-field. The configuration of continents was not so far different from the present but that the principal climatal districts were marked out, and the great flexures of the lines limiting zones of climates were arranged very much as we now know them." On the subject of water-supply the author enters upon a practical question of great moment, and especially interesting to an empire which embraces regions so deficient in rain as are certain parts of Australia and South Africa. What is the influence of agriculture upon climate? Will a cultivated country when brought under cultivation induce a larger amount of atmospheric precipitation than it did when in the state of a *desert*,—not, we must remember, of *forest?* Mr. Gilbert admits that the water of the Great Salt Lake has risen ever since, or nearly ever since, the occupation and cultivation of the neighbouring country by the Mormon settlers. He admits, further, that the rise of the lake indicates an increase of rainfall as compared with evaporation, and that large areas of land brought under cultivation are cooler than desert-surfaces, and hence better able to induce rainfall from currents of moist air. But he maintains that this increased rainfall is due to the increase of evaporation brought about by the wide distribution of water in irrigation, and that there seems no reason to believe that the result in precipitation shall exceed—if indeed it can equal—the expenditure in evaporation. This remark on the influence of irrigation—a practice very extensively resorted

to in the Salt Lake district—has a significance which should not be overlooked. If it, as can scarcely be denied, reduces the temperature of the soil and increases the amount of rainfall, it must certainly have a generally pernicious effect in a country like England, where the temperature of the soil in average seasons might be advantageously increased, and where the rainfall is already so sufficient, in proportion to the evaporation, that any increase would ensure us a succession of wet summers and ruined harvests. It is, however, we think, demonstrated—even to superfluity—that the destruction of the forests has already reduced many regions in Europe, Asia, and Africa, to comparative sterility, and is evidently exercising an injurious effect in many others, as at the Cape. We have ourselves observed drizzling rain falling in a wood, whilst none could be perceived in the open country on all sides. Now if the destruction of vegetation can effect a decrease of the volume of water in the rivers, and, if not, reduce the total amount of rainfall during long periods of time, can still convert it from a regular and useful supply to an alternation of violent storms and long periods of drought, it does seem highly probable that planting should have an inverse result.

The ninth volume of Mr. F. V. Hayden's "Report of the United States Geological Survey of the Territories" is exclusively devoted to the invertebrate palæontology of the cretaceous and tertiary formation of the Upper Missouri country, and represents the result of a wonderful amount of labour and research. This will be at once understood when we say that 130 genera of fossil Mollusca have been examined and characterised, and a typical species of each described. The synonyms of the genera and species have been collated, and as far as possible determined. The illustrations consist of forty-five quarto plates, some of them containing more than thirty figures each and admirably illustrated. The whole work must be pronounced in the highest degree creditable to the geological staff of the United States Government, and as a most valuable contribution to palæontological science.

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